THE
POPULAR SCIENCE
MONTHLY.

FEBRUARY, 1905.

AN ADDRESS ON ASTROPHYSICS.*

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THE investigator in any field of knowledge must, as the price of success, both comprehend the general principles underlying his special problem, and give constant care to its details. Yet it is well, now and then, to leave details behind and consider the bearing of his work upon the science as a whole. Whether our subject is that of determining the accurate positions of the stars, or their radial velocities, the orbits of the planets, or the constitution of the sun, we are making but minor contributions to the solution of the two great problems which at present compose the science of astronomy. These problems, perhaps the most profound in the realm of matter, may be stated thus:

1. A determination of the structure of the sidereal universe; of the form of that portion of limitless space occupied by the universe; of the general arrangement of the sidereal units in space; and of their motions in accordance with the law of gravitation.

2. A determination of the constitution of the nebulæ, stars, planets and other celestial objects; of their physical conditions and relations to each other; of the history of their development, in accordance with the principles of sidereal evolution; and of what the future has in store for them.

The first problem has for its purpose to determine *where* the stars are and whither they are going. It has been ably treated under the head of astrometry.

The second seeks to determine the nature of the heavenly bodies —*what the stars really are.* This field of inquiry is well named, astrophysics.

* Delivered at the St. Louis International Congress of Arts and Science.

The motives of these problems are distinct and definite; but, judged by the ultimate bearing of his results, nearly every astronomer is working in both fields. The astrophysicist borrows the tools of the astronomer of position, the latter uses the results of the former, and *vice versa*. Let me give two illustrations. Astrophysics desires to know the relative radiating power of matter in different types of stars—the Sirian and solar types, for example. The meridian circle and the telescope discovered a companion to Sirius; the micrometer determined the form and position of the orbits; the heliometer observed the star's distance; and the photometer measured the quantity of light received from it. Computations determine from these data that Sirius is but two and one half times as massive as our sun, whereas it radiates twenty-one times as much light; from which it follows that a given quantity of matter in Sirius radiates many times as effectively as the same quantity of solar matter—a fact of prime importance in the astrophysical study of all Sirian stars. The parallaxes of the stars are needed by the student of stellar evolution as well as by the student of the structure of the heavens.

Again, the measurement of radial velocities of the stars has been left almost completely to those observers who are especially interested in astrophysical problems and methods, yet it is the student of astrometry who is eager to use their results. The overlapping of the two departments of astronomy is but the symbol of progress.

The term astrophysics is of the present generation, but the beginnings of astrophysical inquiry are somewhat older. Theories of planetary evolution by Kant and Laplace; observations of nebulæ and star clusters by the elder Herschel, and his wonderfully sagacious deductions concerning them; various studies of planetary markings and conditions; systematic investigations of the sun spots, including Schwabe's discovery of their eleven-year period—these constituted the main body of the science in 1859. But the spirit of inquiry as to the nature of the heavenly bodies was latent in many quarters; and Kirchhoff's immortal discovery of the fundamental principles of spectrum analysis opened a gateway which many were eager to enter. The spectroscope became at once, and has remained, the astrophysicist's principal instrument. However, the spectrum is not his only field, nor the spectroscope his only tool. Radiation in all its aspects, and the instruments for determining its quantity and quality, are the means to the ends in view. And the great generalizations of scientific truth, the doctrines of evolution and of the conservation of energy, for example, have been no less helpful here than elsewhere.

The study of our sun forms the principal basis of astrophysical research. The sun is an ordinary star, comparable in size and condition with millions of other stars, but it is the only one near enough

to show a disk. The point image of a distant star must be studied as an integrated whole; whereas the sun may be observed in considerable geometrical detail. We can not hope to understand the stars in general until we have first made a thorough study of our own star.

We are unable to study the body of the sun, except by indirect methods. The interior is invisible. The spherical body which we popularly speak of as the sun is hidden from view by the opaque photosphere. This photospheric veil, including the sun spots; the brilliant faculæ and flocculi, projecting upward from the photosphere; the reversing layer, in effect immediately overlying the photosphere; the chromosphere, a stratum associated with and overlying the reversing layer; the prominences, apparently ejected from the chromosphere; and the corona, extending outward from the sun in all directions to enormous distances; these superlatively interesting features of the sun constitute the only portions accessible for direct observation; and they are an insignificant part of its mass. They are literally the sun's outcasts. Our knowledge of the sun is based almost exclusively upon a study of these outcasts. Nevertheless, we are able to formulate a fairly simple and satisfactory theory of its constitution.

The materials composing the sun appear to be the same as those forming the earth's crust. Of the eighty known elements, slightly more than half have been observed in the reversing layer and chromosphere, by means of their spectra. The existence of others remains unproved, but there are no reasons to doubt that they too are present. Our most complete study of the sun's composition was made by Rowland, and he has said that if the earth were heated to the temperature of the sun, the terrestrial and solar spectra would be virtually identical.

The force of gravity at the sun's surface is well known, but the radial pressures at interior points are somewhat uncertain, as they depend upon the unknown law of increasing density with increasing depth. The minimum value of the pressure at the sun's center is thought to be fully ten thousand million times the pressure of our atmosphere at sea-level. The most probable value of the effective temperature of the sun's radiating surface is 6000° Centigrade, and the minimum value for the center is perhaps five million degrees. In view of these high temperatures, and the low average density of the sun, the interior must be largely gaseous, and perhaps entirely so; although, under the stupendous pressures, a great central core is probably of a viscous consistency, but ready to assume the usual properties of a gas when the convection currents carry the viscous masses up into regions of lower pressure.

The surface strata are radiating heat into surrounding space. To maintain the supply, it is imperative that convection currents should carry the cooled masses down into the interior, and bring corresponding hot masses up to the surface. These currents make the sun a very

tempestuous body. Further, the outrushing materials must acquire the higher rotational speeds of the surface strata, and the inrushing must lose their tangential momentum; and these can scarcely be ineffective factors in the sun's circulatory system.

The mechanical theory of the maintenance of at least a part of the sun's radiation must be considered as a necessary consequence of the law of gravitation—as unavoidably a consequence of that law as precession is. Helmholtz computed that a contraction of the solar diameter of less than 400 feet per year would suffice to maintain the present rate of flow. Whether this is the sole source of supply is uncertain, and very doubtful. The discovery of sub-atomic forces in uranium, thorium and radium is of interest in this connection. These radio-active substances have revealed the existence of intense forces within the atom, long dreamed of by students of physics and chemistry, but never before realized. The energy radiated by an atom of these substances is thousands of times greater than that represented by the ordinary chemical transformations of equal masses of any known element. Whether these forces are working within the sun, prolonging its life many fold, and incidentally diminishing the required rate of Helmholtzian contraction, we do not know; but we are not justified in treating gravitation as the sole regulator of radiation. We are encouraged to this view by the fact that the age of the earth, as interpreted by geology and biology, is many times greater than the superior limit set by the gravitational theory.

The dazzlingly brilliant photospheric veil which limits the depth of our solar view is due, with no room for doubt, to the condensation of those metallic vapors which, by radiation to cold space, have cooled below their critical temperatures. These clouds form and float in a great sea of uncondensed vapors, very much as do our terrestrial clouds; but it seems probable that the process of formation is continuous and rapid; and that they are added to from above, or from the interstices, and melt away from below.

The sun spots are the most extensively studied and the least understood of all solar phenomena. That they are large-scale interruptions in the photosphere, and at the same time the most striking evidence of atmospheric circulation, there can be no doubt. Observations made near the sun's limb, to determine whether the spots are elevations or depressions with reference to the photosphere, seem not to be reliable, perhaps because of abnormal refractions in the strata overlying and surrounding the spots. In the the earth's atmosphere, a high barometer is the indication of descending currents, which generate heat by compression and prevent cloud formation. Is not the umbra of a spot an area of high pressure, which forces the solar atmosphere slowly downward, preventing cloud formation in that area, but favoring the

growth of brilliant faculæ and flocculi in the regions of uprush surrounding the spot,—a theory first suggested by Secchi?

The visible spots are not the sole evidences of circulation. The surface is covered with a network of interstices, or vents between clouds, which probably exercise all the functions of the visible spots, but on a smaller scale.

There is no reason to question the truth of Young's discovery that the Fraunhofer lines originate in the absorption of a reversing layer—a thin stratum of uncondensed vapors lying immediately over and between the photospheric clouds.

The chromospheric stratum, several thousand miles in thickness, includes and extends far above the reversing layers, and contains the lighter gases, such as hydrogen and helium, and the vapors of calcium, sodium, magnesium and other elements which do not condense under existing temperatures.

The prominences have in general the same composition as the chromosphere. In some the lighter gases, and in others the heavier metallic vapors, predominate. They are portions of the chromosphere projected beyond its usual level by the more violent ascending currents, or perhaps by eruptions of a volcanic character; and these forces are almost certainly augmented by the pressure of the sun's radiation. It is difficult to account for the quiescent, cloud-like prominences in regions far above the chromosphere on any supposition other than that they are in equilibrium under the opposing influences of gravity and radiation pressure.

The nature of the forces which control the general and detailed coronal forms is but little understood. Motion within the corona has never been directly observed. Yet we can not question that the component particles are driven outward from the sun, and that many of them probably fall back into the sun, either singly or after combining to form larger masses. It is suggested that out-bound particles may be started on their way by the violent solar circulation, continued on their journey by radiation pressure, and arranged in the characteristic streamers under the influence of magnetic forces.

The light received from the corona is of three kinds:

1. A small quantity of bright-line radiations from a gas overlying the chromosphere. This gas is unknown to terrestrial chemistry, and astronomers provisionally call it coronium. It is distributed very irregularly over the solar sphere, and shows a decided preference for the sun-spot zone.

2. The bright-line radiations from coronium are almost a negligible quantity, in comparison with those from the same regions which form a strictly continuous spectrum, and which seem to be due to the incandescence of minute particles heated by the intense thermal radiations from the sun.

3. A small proportion of the inner, and a large proportion of the outer, coronal light are solar rays reflected and diffracted by the coronal particles.

Arrhenius has recently shown that Abbot's observation of an apparent temperature of the corona nearly equal to that of his observing room is in harmony with the spectrographic evidence of an inner corona composed of incandescent particles. Arrhenius finds that one minute dust particle to each 11 cubic meters of space in the coronal region observed by Abbot, raised to the temperature of 4620° absolute required by Stefan's law, would give a corona of the observed brightness, and of the observed temperature. The bolometric strip measured the resultant temperature of the few highly-heated particles and the cold background of space upon which the particles are seen in projection.

Arrhenius further estimates that a corona composed of incandescent dust particles need not have a total mass greater than 25,000,000 tons, to radiate the quantity of light yielded by the brightest corona observed. This is approximately that of a cube of granite only 200 meters on each side; a remarkably small mass for a volume whose linear dimensions are millions of kilometers.

This *résumé* of solar theory necessarily overlooks many unsettled points of great significance. Most important of all, perhaps, is that of the solar constant: does it vary, and in accordance with what law? Why is there a sun-spot period, and why are the large spots grouped within limited zones? Why does the form of the corona vary in a period equal in length to the spot period? Why does the angular speed of rotation increase from the poles to the equator? What is the origin of the faculæ and the flocculi? Why do the Fraunhofer lines show little evidence of high atmospheric pressure? Why are the radiations from calcium, one of the heavy elements, so prominent in the higher chromospheric strata and in the prominences? A great number of such questions are pressing for solution. Under the stimulus of the brilliant researches of our chairman, the reinventor and the leading developer of the spectroheliograph, cooperative plans for solar work on a large scale are now being organized. We should be vitally interested in promoting these plans; for the study of the sun, as the principal foundation of astrophysical research, has been unduly neglected.

The celestial bodies develop under conditions over which we have no control. We must observe the facts as they are, at long range, and interpret them in accordance with those principles of physical science which govern what seem to be closely related terrestrial phenomena. A successful study of the development of matter in distant space, under the influence of heat, pressure, electricity and other forces of nature

demands a complete understanding of the action of the same forces upon terrestrial matter. The astrophysicist dwells in the laboratory as well as in the observatory; and laboratory researches must supply the links which connect world life and star life.

It has not been possible for laboratory investigators to reproduce stellar phenomena on a scale approaching that occurring in nature, nor to duplicate conditions of temperature and pressure existing within the stars; and these are unfortunate limitations. Nevertheless, many successes have been achieved in this direction. The low-temperature triumphs of Dewar, Olczewski and others approximate to the conditions of space surrounding the stars. The electric arc and spark appear to reproduce the temperatures of many stellar chromospheres and reversing layers. The electric furnace of Moissan seems to supply temperatures comparable with those of the photosphere, and it promises to throw light upon the processes of cloud formation in the stars. Investigations as to the influence of varying pressures—from almost perfect vacua up to many atmospheres—as to the effects of varying electrical conditions and of other factors have answered many celestial questions, and introduced others equally pressing.

Laboratory observations have established that the spectra of the elements are not the same under all circumstances. We formerly thought it remarkable that nitrogen should have two or three characteristic spectra, or that a metal should have a spark spectrum and an arc spectrum. We are now confronted with the potent fact that an element may have a variety of spectra, depending upon the nature and the intensity of the forces employed in rendering it luminous. But for most cases these involve only moderate variations in the relative intensities of spectral lines. The complications which threaten to result therefrom are more apparent than real. The multiplicity of spectral reactions promises to be a powerful aid to analysis, by supplying a more exact key to the conditions in the celestial light source which produce the observed effects.

For many years following the application of the spectroscope to celestial problems it was supposed that a continuous spectrum must indicate incandescent solid or liquid matter. The situation is not so simple as this. Some gases radiating under high pressures give spectra apparently continuous.

The effect of increasing temperature conditions on certain spectra has long been well known. Certain lines are enhanced in relative brilliancy when we pass from the temperature of the arc to that of the high-tension spark, and *vice versa;* but it seems certain that, within measurable limits, the positions of the lines do not change under this influence.

Humphreys and Mohler have proved that the spectral lines are shifted by pressure;—toward the red with increasing pressure in the

atmosphere surounding the arc. It is not difficult to see the bearing of this discovery upon astrophysical inquiry. Some subjects are made more complex; but the hope is held out that eventually we may detect these indications of pressure, differentially, in the brighter stars.

It is also known that the spectra of some elements are altered by the presence of other elements, but the extent and character of the induced changes are little understood. As the chemical elements are never found alone in celestial bodies, the serious consequences of this effect must be evident.

The temperature in glowing Plücker tubes is of great interest, from its bearing upon the probable temperatures of nebulæ, the auroræ and other bright-line phenomena of a diffuse nature. It is not certain that direct observation by any thermometric device can deal with the problem. The measures thus far attempted have assigned temperatures but a few degrees higher than that of the environment. These indications are probably correct for the average temperature of the contents of the tube, but hardly so for those molecules which are glowing. It has been suggested that perhaps a very small proportion of the molecules receive and carry the discharge; that while the molecules in action may be very hot, the average for all in the tube is very low. It seems reasonable to suppose, also, that the low-temperature indication is due to the fact that the current is actually passing but a small fraction of the time. The effect upon the eye is that of a continuous glow, whereas the thermometer measures the average effect.

The influence of a magnetic field upon the character of spectral lines, established in the laboratory by Zeeman, has not yet been observed in celestial spectra, but its detection may be merely a question of the dispersive power available on faint spectra.

It will be perceived that the interpretation of celestial spectra must be made with circumspection. We are not always justified in reaching conclusions upon the spectroscopic evidence alone; general conditions must also be taken into account. For example, shall we say that the temperature of the gaseous nebulæ is very high, because they have bright-line spectra? On the contrary, the difficulty of maintaining a high temperature in a mass so attenuated should be given at least equal weight. The radiating molecules or particles may for the instant be quite hot, but the effective temperature of the whole nebula is probably low.

The experimental verification of radiation pressure by Lebedew, and by Nichols and Hull, is far-reaching in its consequences. We must take this force into account, as truly and as constantly as we must consider gravitation. Radiation pressure requires us to reconstruct our theories of comets' tails, of the corona, of the zodiacal light, of

the auroræ,—in fact of every phenomenon of nature involving minute particles. And what celestial object does not involve them?

On the other hand, the student of the stars has pointed the way for the laboratory investigator, in many instances. The ultra-violet hydrogen series was photographed by Huggins, in the spectrum of Vega, before it was found in the laboratory; and Pickering has discovered another hydrogen series, in Zeta Puppis, which still awaits terrestrial duplication. The hypothetical element, helium, in the sun, waited a quarter-century for Ramsay's discovery, and the laboratory investigation of its more complete spectrum which followed. Students of the solar corona and of the gaseous nebulæ are discussing the properties of the hypothetical elements coronium and nebulium almost as familiarly as if they had actually handled them. Out of some 20,000 absorption lines mapped by Rowland, more than the half are awaiting laboratory identification.

In this connection, the mathematical relations existing between the positions of lines in the spectra of many of the principal elements, discovered by Balmer, Kayser, Runge and Paschen, have already been of great utility; and they can scarcely fail to illuminate the question of the construction of the atoms involved.

A new era of physical science was inaugurated about eight years ago by the discovery of argon on the one hand, and of the X-rays on the other. The former was followed by the discovery, in quick succession, of several other constituents of the earth's atmosphere which at present demand our attention as to their presence in chromospheric and auroral phenomena. It would be most surprising if the many forms of radiation, including those of the radio-active substances, discovered in the train of the X-rays, should not throw strong light upon the constitution of matter. And how shall we deal intelligently with the forms of matter in other worlds before we understand the constitution of matter upon the earth? The modern theory of electrons, in which material atoms play the subordinate part, and electric charges the principal part, promises to have a wide application to celestial phenomena. Further, the actual transport and interchange of matter in the form of small particles, from one star to another, as urged with great learning and skill by Arrhenius, seems to be a plain and unavoidable consequence of recently established physical facts. Should this theory stand the test of time, its far-reaching consequences would accord it a position of the first rank.

The photographic program inaugurated with the Crossley Reflector by Keeler comprised 104 negatives of the regions containing the principal nebulæ and star clusters. These photographs, covering but one six-hundredth part of the entire sky, record 850 nebulæ, of which 746 are new. If this proportion should hold good over the whole sphere, the

number discoverable with this instrument, with exposures of ordinary length, would be half a million. This estimate would be too large, in case the smaller nebulæ have a tendency to cluster around the prominent nebulæ, which to some extent is probably true. The number of stars visible in our great telescopes is of the order of one hundred millions. The dark or invisible bodies indicated by several considerations—the planets in the solar system, the spectroscopic binaries, the eclipsing variable stars, and the gravitational power of the universe—should outnumber the bright ones several fold. It is the thesis of astrophysics that all these objects—the nebulæ, the bright stars and the invisible bodies—are related products of a system of sidereal evolution. The general course of the evolutionary process, as applied to the principal classes of celestial objects, is already known. We are able to group these classes, with little chance of serious error, in the order of their effective ages.

The earliest form of material life known to us is that of the gaseous nebulæ. In accordance with the simplest of physical laws, a nebula must radiate its heat to surrounding space. In accordance with another law, equally simple, it must contract in volume—toward a center, or toward several nuclei—and generate additional heat in the process. Eventually a form of considerable regularity will result. Whether this form is that of a typical planetary nebula, of a spiral nebula, or of some other type, is a matter of detail. It is quite possible that nature uses several molds in shaping the contracting masses, according as they lie on one side or the other of critical conditions. The variety of existing forms is extensive. One can see very little resemblance in the Trifid Nebula, which is apparently breaking up into irregular masses; the Dumb Bell Nebula, from whose nearly circular form rings of matter seem to be separating; the great spiral nebulæ; the Ring Nebula in Lyra, with a central star; the compact planetary nebula G. C. 4390, containing a dense, well-defined nucleus; and many others of distinct types.

The condensed globular forms occupying the positions of nebular nuclei have almost reached the first stage of stellar life.

It is not difficult to select a long list of well-known stars which can not be far removed from nebular conditions. These are the stars containing both the Huggins and the Pickering series of bright hydrogen lines, the bright lines of helium, and a few others not yet identified. Gamma Argus and Zeta Puppis are of this class. Another is DM. + 30.°3639, which is actually surrounded with a spherical atmosphere of hydrogen, some five seconds of arc in diameter. A little further removed from the nebular state are the stars containing both bright and dark hydrogen lines;—caught, so to speak, in the act of changing from bright-line to dark-line stars. Gamma Cassiopeiae, Pleione and Mu Centauri are examples. Closely related

to the foregoing are the helium stars. Their absorption lines include the Huggins hydrogen series complete, a score or more of the conspicuous helium lines, frequently a few of the Pickering hydrogen series, and usually some inconspicuous metallic lines. Calcium absorption is absent, or scarcely noticeable. The white stars in Orion and the Pleiades are typical of this age.

The causes which produce bright lines in stars are not thoroughly understood; but atmospheres of higher temperatures than their underlying strata, or very extensive simple atmospheres, seem to be demanded. The former condition, on the large scale required, involves some difficulties, and mildly suggests the possibility that external influences may be acting upon the radiating strata of bright-line stars.

The assignment of the foregoing types to an early place in stellar life was first made upon the evidence of the spectroscope. The photographic discovery of nebulous masses in the regions of a large proportion of the bright-line and helium stars affords extremely strong confirmation of their youth. Who that has seen the nebulous background of Orion, or the remnants of nebulosity in which the individual stars of the Pleiades are immersed, can doubt that the stars in these groups are of recent formation?

With the lapse of time, stellar heat radiates into space; and, so far as the individual star is concerned, is lost. On the other hand, the force of gravity in the surface strata increases. The inevitable contraction in volume is accompanied by increasing average temperature. Changes in the spectrum are the necessary consequence. The second hydrogen series vanishes, the ordinary hydrogen absorption is intensified, the helium lines become indistinct, and calcium and iron absorptions begin to assert themselves. Vega and Sirius are conspicuous examples of this period. Increasing age gradually robs the hydrogen lines of their importance, the H and K lines broaden, the metallic lines develop, the bluish-white color fades in the direction of the yellow, and, after passing through types exemplified by many well-known stars, the solar stage is reached. The reversing layer in solar stars represents but four or five hydrogen absorption lines of moderate intensity; the calcium lines are commandingly prominent; and some 20,000 metallic lines are observable. The solar type seems to lie near the summit of stellar life. The average temperature of the mass must be nearly a maximum, for the low density indicates a constitution that is still gaseous.

Passing time brings a lowering of average temperature. The color passes from yellow to the red, in consequence of lower radiating temperatures and increasing general absorption by the atmosphere. The hydrogen lines become indistinct, metallic absorption remains prominent, and broad absorption bands are introduced. In one type, of which Alpha Herculis is an example, these bands are of unknown

origin; in another, illustrated by 19 Piscium, they have been definitely identified as of carbon origin. The relation between the two types is not clear. It has even been advocated that the evolutionary process divides shortly after passing the solar stage; that the reddish stars with absorption bands sharply terminated on the violet edges are on one branch, and that the very red stars with absorption bands sharply defined on the red edges are on the other branch. This plan of overcoming a difficulty seems to me to introduce a greater difficulty; and I do not doubt that systematic investigation will supply the connections now missing. That the denser edges of the bands in Type IV (Secchi) should occupy the same positions as the denser edges of absorption bands in Type III, can hardly be without significance; and Keeler's view that the carbon absorption bands in Type IV are matched by carbon radiation in some stars, at least, of Type III suggests a most promising line of investigation for powerful instruments.

There is scarcely room for doubt that these types of stars are approaching the last stages of stellar development. Surface temperatures have lowered to the point of permitting more complex chemical combinations than those in the sun. The development of 'sun spots' on a large scale is quite probable, and the first struggles to form a crust may be enacted. Type III includes the several hundred long-period variable stars of the Omicron Ceti class, whose spectra at maximum brilliancy show several bright lines of hydrogen and other elements. The hot gases and vapors seem to be alternately imprisoned and released. It is significant that the dull red stars are all very faint;—there are none brighter than the 5½ magnitude. Their effective radiating power is undoubtedly very low.

The period of development succeeding the red-star age of Type IV has illustrations near at hand, in the planets Jupiter and the earth; invisible save by borrowed light. When the interior heat of a body shall have become impotent, the future promises nothing save the slow leveling influence of its own gravitation and meteorological elements. It is true that a collision may occur to transform a dark body's energy of motion into heat, sufficient to convert it into a glowing nebula, and start it once more over the long path of evolution. This is a beautiful theory, but the facts of observation do not give it satisfactory support. There is little doubt that the principal novæ of recent years have been the results of collisions, either between two massive dark bodies, or between a massive body and an invisible nebula. The suddenness with which intense brilliancy is generated would seem to call for the former, but the latter is much more probable, in view of many facts. The nebular spectra of the novæ are generated in a few months; but in every case thus far observed the bright nebular bands grow faint very rapidly, and in the course of a few years leave a continuous spectrum,—apparently that of an ordinary star. Either the masses

involved in the phenomena are extremely small, or the disturbances are but skin-deep. In any case, the novæ afford little evidence as to the complete re-nebularization of dark bodies.

I spoke of the *average* temperature of a developing star as reaching a maximum near the solar stage, when the border-line between gaseous and liquid constitution is reached. This refers to the entire mass. The law of surface temperatures is quite a different one. The bright-line and helium stars seem to have hotter surfaces than the solar and red stars. The spectra which we observe are surface phenomena which indicate the temperatures of the radiating and absorbing strata. The maximum intensity of continuous radiations is higher up in the spectrum for the white stars than for the yellow and red, a safe indication of higher temperatures. The lines in white-star spectra are distinctly the enhanced lines thought to be produced by high temperatures. These facts are not inharmonious. Surface temperature is a function of the rapidity with which convection currents can carry heat from the interior to the surface. The comparatively low internal heat of white stars, delivered quickly at the surface by rapidly moving gases, may readily maintain higher atmospheric temperatures than the much hotter interiors of solar stars, whose circulation has the sluggishness of viscosity.

Sir William and Lady Huggins are inclined to assign greater importance to mass and density, as factors in evolution, than to temperatures. Their view is that under the influence of great surface gravity, the generation and radiation of heat is accelerated, and the life of the star is lived more rapidly. They have been led to this view, in part, by the apparent anomaly of double stars, in which the more massive primary is generally yellower than the less massive companion. The subject is one of great difficulty and importance, and, unfortunately, laboratory methods are on too small a scale of mass and pressure to solve the problem.

Up to the year 1800 only twelve variable stars were known. Chandler's catalogue dated 1888 contains 225 entries. The remarkable progress made by astronomical science in the past fifteen years is fairly indicated by the fact that in this interval the number of known variable stars increased from 225 to more than 1400. To Harvard College Observatory belongs the great credit of discovering nearly 900 of these objects.

In many respects variable stars constitute the most interesting class of objects in the heavens. The tens of millions of ordinary stars are undoubtedly growing older; and the tens of thousands of nebulæ, from which stars will eventually be formed by processes of condensation, are undergoing transformation; but appreciable changes in the ordinary stars and in the nebulæ proceed with extreme deliberation,

and no permanent changes have yet been noted. Variable stars, on the contrary, are changing before our eyes; and they repeat their fluctuations continually. They present opportunities for discoveries of the greatest interest in themselves, and of remarkable utility in the study of the problem of stellar evolution.

It is a conservative statement that in nineteen variable stars out of twenty we have little idea as to the causes of variability. The causes of the variations have been determined in the case of Algol and a few others of that class: large dark companions revolve around these stars, and once in every revolution the companions pass between us and the principal stars, thus preventing a portion of their light from reaching us. In Zeta Geminorum and three or four others of its class the spectroscope has shown that massive dark companions are close to, and rapidly revolving around, the principal stars. These invisible companions produce disturbances in the extensive atmospheres of the stars, and cause the observed variations in brightness; but the nature of the disturbances is still a matter of conjecture. Omicron Ceti and other stars of its class have given no evidence of companions. Brightness variations in them seem to be due to internal causes. Perhaps they have reached the age when solid crusts attempt to form on their surfaces, just as one day a crust struggled to form on the liquid earth. A crust formed one month may be melted or sink to a lower level a few months later. Perhaps there are 'sun-spots' on these stars, in scale vastly more extensive and in period shorter than those on our sun; but these suggested explanations may be far from the truth.

For more than half a century a great many astronomers have devoted themselves assiduously to making photometric observations of variable stars. There are a dozen observatories, both large and small, which are systematically devoting some of their resources to this work. By common consent of the profession, or by appointment from learned societies, there have for some fifty years been individual astronomers, or committees of astronomers, who systematize results, call attention to the need for observations of certain neglected objects, and in many other ways encourage the photometric study of variable stars. Photometers are inexpensive, the methods are simple, and results have rapidly accumulated.

Observations of variable stars with slit-spectrographs, on the contrary, are surprisingly meager and fragmentary. Not a single institution, not a single telescope, not a single observer, is working continuously or even extensively on the subject. Yet the method is a very powerful one: the few isolated studies made on variable stars have led to results of remarkable richness. The subject is one of great difficulty. Photographic spectra require much time for accurate measurement and reduction. And, finally, powerful and expensive instruments are demanded.

Harvard College Observatory has been remarkably successful in discovering variable stars by means of peculiarities in their spectra, as well as in classifying them, and in qualitative studies of many spectral details, using objective-prism spectrographs; but it is hoped that slit spectrographs, attached to powerful telescopes, may soon be devoted systematically to this subject, as it constitutes one of the richest fields now awaiting development.

A century and a half of meridian-circle observations has given to the world, as one of many priceless contributions, a knowledge of the proper motions of several thousand stars. Some of the ablest astronomers have used these results as a basis for determining the most probable elements of the sun's motion, and in studies upon the distribution of the stars in space. Unfortunately, these investigations necessarily involve assumptions as to the unknown distances of the stars.

A few years following the application of the spectroscope to the study of celestial objects, Huggins recognized that the Doppler-Fizeau principle supplied, in theory at least, the long hoped-for method of measuring the components of stellar motions in the line of sight—their radial velocities; and that the application of this method would enable us to determine both the direction and the speed of the solar motion, entirely independently of the distances of the stars. Efforts to apply this method met with signal failure for twenty years, and doubts even as to ultimate success were quite generally felt and freely expressed. The beginnings of success were made by Huggins and Pickering, in showing that photography reveals, with great clearness, the delicate spectral lines which the eye in purely visual observations is unable to see at all. In 1888, Vogel applied this knowledge in the first photographic attempt to measure radial velocities, and his work inaugurated a new era. His observations, obtained with a small telescope and imperfect spectrograph, were not sufficiently accurate to meet the needs of the principal sidereal problems, but they led to several brilliant discoveries at Potsdam, and were invaluable in marking out the path of progress. It was not until 1896 that the use of a powerful telescope, equipped with an efficient spectrograph, gave results accurate enough to satisfy present requirements. In fact, the accuracy obtained exceeded our most hopeful expectations.

It is not surprising that thirty years were required to develop successful methods. The work is so delicate that, unless suitable precautions are taken at every point in the process, the errors introduced may readily be larger than the quantities sought for. With the Mills spectrograph, for example, a speed of nine kilometers per second displaces the lines only 0.01 mm. The probable error of a velocity determination for the best stars, such as Polaris, is but one fourth of a kilometer per second, corresponding to a linear displacement of 0.0003

mm., or 0.00001 inch. In view of the newness of the subject, the richness of the field, and the fact that the more active great telescopes are now nearly all applied to this work, I append a list of the improvements which have contributed most powerfully to recent progress:

1. A realization of the fact that a spectrograph is an instrument complete in itself. The telescope to which it is attached serves only to collect the light and to deliver it properly upon the slit.

2. The development of a method of reduction which permits the use of all good stellar lines, irrespective of whether they correspond to, or lie between, the comparison lines.

3. The use of a longer collimator, permitting a wider slit, and requiring larger prisms, with greater resolving power.

4. The use of simple prisms, of better glass, with better optical surfaces.

5. Care in collimating, to insure that the star light and comparison light traverse identically the same part of the collimator lens.

6. The adoption of a compact and rigid form of spectrograph mounting, designed in accordance with good engineering practise.

7. The elimination of flexure effects by supporting the spectrograph, in connection with the telescope, in accordance with engineering principles. The conventional spectrograph had been supported entirely at its extreme upper end; the instrument projected out into space, unsupported, boldly inviting flexure under the varying component of gravity.

8. The use of a constant temperature case around the instrument.

9. Precautions taken to eliminate many sources of error from the measures of the spectrograms.

Up to December, 1900—the last month of the departing century—the speeds of 325 stars had been determined with the Mills Spectrograph in the northern two thirds of the sky. Omitting several stars whose lines could not be measured accurately, and some thirty spectrographic and visual binaries for whose centers of mass the velocities were still unknown, 280 stars remained available for deducing the relative motion of our solar system. The observational data were distributed symmetrically in right ascension, and the result for this coordinate of the apex agreed with Newcomb's proper-motion result within a small fraction of a minute of arc. The data were extremely unsymmetrical in declination, as there were few observations between $-15°$ and $-30°$ declination, and none whatever south of $-30°$. The solution placed the apex 15° south of Newcomb's position. The deduced speed, 20 km. per second, is no doubt close to its true value.

There is a question whether the direction of the solar motion can be determined more accurately from proper motions or from radial velocities, an equal number of stars being available in the two cases; but as to the speed, no doubt of the very marked superiority of the spectrographic method can exist. This, however, is but incidental,

for the two methods are in fact mutually helpful and mutually dependent: the motion of every star involves both components.

In this connection two points call for appreciation: First, the motion of the solar system is a purely relative quantity. It refers to the group of stars used in the solution. We could easily select twenty or thirty of these stars whose velocities were such that the deduced motion would be reversed 180° from that given by the entire list of stars. We want to know the solar motion with reference to the entire sidereal system. A satisfactory solution of the problem demands that we use enough stars to be considered as representative of the whole system. Second, the great sidereal problems require that observational data for their solution should cover the whole sky. Until one year ago radial velocity measures were confined to the northern two thirds of the celestial sphere. Further attempts to deduce the solar motion from northern observation alone would not be justified. Observations in the southern third of the sky were needed, not only to represent that large region in the solution, but in order that the unknown systematic errors which affect the northern observations, as well as the southern, might be eliminated, through the symmetrical balancing of the material. Fortunately the energetic and wise policy of the Cape Observatory and the generosity of Mr. D. O. Mills have provided two complete equipments, which are now busily engaged in supplying the southern data required. The Mills spectrograph in the northern hemisphere has secured about three thousand spectrograms of approximately five hundred stars, and the Mills spectrograph in the southern hemisphere has secured four hundred spectrograms of one hundred and twenty-five stars. The number of stars not on the Mills list, and *accurately observed* with other high-dispersion spectrographs, is not known, but it is probably between one hundred and two hundred. We may reasonably expect that, in two or three years, as many as eight hundred well-determined radial velocities may be brought to bear upon pressing sidereal problems.

It is a frequent question: Is the solar system moving in a simple orbit, and will it eventually return to the part of its orbit where it is now? The idea of an affirmative answer to this question is very prevalent in the human mind. It is natural to think that we must be moving on a great curve, perhaps closed like an ellipse, or open like a parabola, the center of mass of the universe being at the curve's principal focus. The attraction which any individual star is exerting upon us is certainly very slight, owing to its enormous distance; and the combined attractions of all the stars may not be very much greater; for since we are somewhere near the center of our stellar system, the attractions of the stars in the various directions should nearly neutralize one another. Even though we may be following a definite curve at

the present time, there is, in my opinion, little doubt that we should be prevented from continuing upon it indefinitely. In the course of our travels we should be carried, sooner or later, quite close to some individual star whose attraction would be vastly more powerful than that of all the other stars combined. This would draw us from our present curve and cause us to follow a different one. At a later date, our travels would carry us into the sphere of attraction of some other great sun which would send us away in a still different direction. Thus our path should in time be made up of a succession of unrelated curves.

Spectroscopic binary systems, as by-products of radial velocity measurements, are of exceedingly great interest, from the light which they cast upon the construction of other systems than ours. When we look at the sky on a clear night, we may be sure that at least one star in six or seven is attended by an invisible companion, comparable in mass with the primary body, the two revolving around their common center in periods varying from two or three days in many cases, up to three or more years in others. For the triple system of Polaris the long period perhaps exceeds fifteen or twenty years. As the shortest-period visual binary now known, that of δ Equulei, is only 5.8 years, the gap between visual and spectroscopic binaries has been definitely closed.

The companions of binaries discovered by means of the spectrograph have not been observed visually in our powerful telescopes, although they have been carefully searched for. They may be so close to the principal star that, viewed from our distance, the two images can not be resolved. The separation of the components is probably less than one hundredth of a second of arc for most of the binaries thus far announced. Again, for very few of the systems are the spectra of both components recorded. This does not establish that the companion is a dark body, but only that it is at least one or two photographic magnitudes fainter than the primary. The fourth-magnitude companion of a second-magnitude star would scarcely be able to impress its lines upon the primary's spectrum. The invisible components in many spectroscopic binaries might be conspicuous stars, if they stood alone.

Only those systems have been detected whose periods are relatively short, and for which the variations of radial speed are considerable. The smallest observed variation is that of Polaris—six kilometers per second. Had the variation for Polaris been only one kilometer, it would no doubt have escaped detection. Such a variation could be measured by present instruments and methods; but this range would not have excited the observer's suspicion, and the discovery would have remained for the future. It is probable that there are more systems with variations of speed under six kilometers than there are with larger ones; and all such are awaiting discovery. The velocity of our sun through space varies slightly, because it is attended by com-

panions—very minute ones compared with the invisible bodies discovered in spectroscopic binaries. It is revolving around the center of mass of itself and its planets and their moons. Its orbit around this center is small, and the orbital speed very slight. The total range of speed is but three one hundredths of a kilometer per second. An observer favorably situated in another system, provided with instruments enabling him to measure speeds with absolute accuracy, could detect this variation, and in time say that our sun is attended by planets. At present, terrestrial observers have not the power to measure such minute variations. As the accuracy attainable improves with experience, the proportional number of spectroscopic binaries discovered will undoubtedly be enormously increased. In fact, the star which seems not to be attended by dark companions may be the rare exception. There is the further possibility that the stars attended by massive companions, rather than by small planets, are in a decided majority; suggesting, at least, that our solar system may prove to be an extreme type of system, rather than a common or average type.

Observations of stellar motions in the line of sight enable us to solve many other important auxiliary problems. Only one will be referred to here. The determination of stellar distances is exceedingly important, and correspondingly difficult. We know the fairly accurate distances of a dozen stars, and the roughly approximate distances of two or three dozen others. Radial velocity observations, in combination with proper motions, will enable us to determine the average distances of entire classes of stars. Let us consider the stars of the fifth magnitude, of which there are a thousand or more. They travel in practically all directions. A definite relation will exist between their average proper motion and their average radial motion, within a small limit of error. If meridian observations ascertain that the average annual proper motion of these fifth-magnitude stars is 0.03 seconds of arc, and spectrographic observations determine that their average speed in the line of sight is thirty-five kilometers per second, it is a simple matter to compute what their average distance must be in order to harmonize the two components.

A study of 280 observed stars as to the relation existing between visual magnitude and velocity in space led to interesting results. The average speed of 47 stars brighter than the third magnitude is 26 km.; of 112 stars between the third and fourth magnitude, 32 km.; and of 121 stars fainter than the fourth magnitude, 39 km. The progression in these results is very pronounced, and I think we are justified in drawing the important conclusion that, on the average, the faint stars of the system are moving more rapidly than the bright stars. This interesting indication should be confirmed or disproved by the use of a much greater number of stars.

The proper method of combining radial velocities for statistical

purposes is a question of great importance. The method of least-squares is based upon the assumption that the accidental errors of observation follow a certain law, found by experience to be substantially true. This method is not applicable to the combination of radial velocities, unless radial velocities are distributed in accordance with the law of accidental errors. Do stellar velocities whose values are near zero exist in greatest numbers? Or does some moderate speed predominate? The average speed *in space* of the 280 stars observed spectrographically is 34 km. When a much greater number of radial velocities is available, the law of distribution must be investigated, and a safe method of combination be developed.

Other practical questions exist as to the proper weights to assign to results of different degrees of accuracy, when it is desired to combine them statistically. The speeds of the brighter second- and third-type stars can be determined well within a kilometer per second, whereas the speeds of first-type stars, containing only broad and hazy lines, may be in error from five to fifteen kilometers. Again, low dispersion spectrography is developing so rapidly that in a few years the speeds of hundreds of the fainter stars will be known within two kilometers. Shall the weights assigned to individual results be proportional to the inverse squares of their probable errors? I think not. The deduced solar motion, for example, should refer to an observed program of stars which shall be representative of the entire sidereal system. It must refer to a star with hazy lines, or to a faint star, as truly as to a bright solar-type star. One poorly determined result for velocity, used alone, should have small weight, but a large number of such determinations should be given considerable weight; proper care being taken to avoid systematic error. Prudence would suggest that separate solutions be made, first for the stars whose spectra admit of accurate measurement, and later for those whose spectra contain hazy lines, or which have been observed with low dispersion. From these a guide as to the relative weights to be assigned to the three or more classes of stars in combination may be found.

Radial velocity observers are concerned as to the part played in the results by pressure in the reversing layers of the stars. The differential effects of pressure are too small to detect in stellar spectra by present means, and there is no known method of eliminating them. We have no recourse but to assume that the stellar lines, neglecting the effect of radial motion, are in identically the same position as the solar lines and the laboratory lines of the elements. Whether the lines in the blue stars are produced under lower pressure than those in the sun, and the lines in the red stars under greater pressure than those in the sun, remains unknown, but this is not impossible. The effect of systematic errors in observed speeds from this source, as well as from

other sources, would be eliminated from many statistical inquiries by having all parts of the sky represented in the solution.

Errors in the tables of absolute wave-lengths do not enter into radial-velocity results, provided the relative values are correct. In fact we scarcely need to know the wave-lengths at all, for the determinations of velocity may be put upon a strictly differential basis, and I incline strongly to the belief that this should be done. Let us consider the case very briefly. Rowland's wave-lengths are based upon spectrograms taken with high dispersion and resolving power. Radial-velocity spectrograms are secured with instruments of much lower power. Close solar and laboratory lines, of different intensities, clearly separated on Rowland's plates, are blended on stellar plates. For this and other reasons, the effective wave-lengths on the two classes of plates are different. The difficulty of assigning correct wave-lengths in the case of plates taken with a single-prism spectrograph is even greater: whole groups of separate lines are blended into one apparent line, and lines actually single are very few indeed. It is necessary to use blends, both in the stellar and comparison spectra. Two methods at least are available to eliminate errors in velocity due to errors in assumed wave-lengths. First: At the conclusion of a long series of observations of stars of the same spectral type, the velocity yielded by each line for each star should be tabulated. If one line gives velocities consistently large or consistently small, the conclusion is that its effective wave-length has been wrongly assumed, and we should be justified in changing it arbitrarily. And so on, for each line employed. This involves the assumption that the comparison bright-lines and the corresponding stellar lines have the same wave-lengths; and all the wave-lengths are reduced to one system, true for the particular spectrograph employed. The method is not entirely free from objection. Second: If the solar spectrum and the comparison spectra are photographed on one and the same plate, under precisely the usual observing conditions, measures of this plate, corrected for the observer's very slight radial velocity with reference to the sun, will form a reduction curve of zero velocity, expressed in terms of micrometer readings. If a spectrogram of star and comparison, made with the same instrument and measured in the same manner, is compared with this reduction curve, measure for measure, the speed of the star will be obtained directly, and irrespective of wave-length values; and many other fruitful sources of systematic error will be eliminated at the same time. Mr. R. H. Curtiss, of Mount Hamilton, formulated a method on this basis last year, and he has applied it to a specroscopic-binary variable star. The observations were made with a spectrograph whose dispersion is but one fifth, and whose exposure time for a given star is but one tenth that of the Mills spectrograph. The probable error for a faint star seems to be not more than twice as great as that for a

bright star with the Mills spectrograph. The method promises to be of great utility, capable of application to several thousand stars between the fifth and eighth magnitudes.

On account of the large proportion of spectroscopic binaries, stars should not be used statistically until observations covering several years have established the constancy of their motions. To determine the orbits and the speeds of the centers of mass of the binary systems, from twenty-five or more spectrograms each, is a task several fold more extensive than that of measuring the constant speeds of the non-binary stars.

There remains the question of cooperation, on the part of radial-velocity observers, to avoid useless duplication, and to increase the output of results. Seven leading observatories in the northern hemisphere, and one in the southern, are in this field, presumably with the intention of remaining indefinitely. A second observatory in the southern hemisphere, devoted exclusively to this work, is of an expeditionary character, and its long continuance is problematical. It is fair to the participating observatories to say, judging by results thus far published, that some are still in the period of experiment and development; and, in fact, that all observers are introducing frequent improvements, which lead to greater accuracy. As long as the development of instruments and methods is in rapid progress, formal cooperation is unwise. Premature cooperation leads to confusion. Duplication of observations for the principal stars is as valuable and desirable in radial-velocity measurements as in meridian determinations of stellar positions. But just as soon as the methods assume a reasonably stable form, the entire sky should be apportioned amongst the interested observatories, in accordance with carefully considered plans which shall permit and encourage individual initiative. I have little doubt that this point will be reached, by a sufficient number of observatories, within two years, and that it would be well to conclude the preliminary organization of cooperative plans within the coming year. Such plans should be formed with severe deliberation, as the labor involved would be commensurate with that devoted to the construction of the Astronomische Gesellschaft Zones for the entire sky.

The problems immediately confronting the astrophysicists of the twentieth century are serious ones. They call for our best efforts. The volume of work demanded is stupendous, and the difficulties to be overcome are correspondingly great. Nevertheless, the men and the means will be forthcoming. The mass of solid fact brought within the realm of knowledge by astronomers now living, many of whom are happily with us this week, is sufficient indication that the general solution of the problems of to-day is but a question of time. And we should be equally hopeful as to the problems of the future, for the desire to know the truth about the universe which surrounds us is an enduring element in human nature.

THE METRIC SYSTEM OF WEIGHTS AND MEASURES.

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IN this age the knowledge of arithmetic is so widespread that it is difficult to conceive that it is comparatively young. The three R's, Reading, 'Riting and 'Rithmetic, seems to our mind almost axiomatic and fundamental in regard to education. It is, therefore, very interesting to read in histories of arithmetic that the earliest known book which contains a systematic exposition of the decimal system of numeration was written in the seventh century, and that our familiar arabic numerals 1, 2, 3, 4, 5, 6, 7, 8, 9, 0 can not be traced to an earlier century than the seventh. According to Ball's 'Short Account of the History of Mathematics,' the arabic arithmetic was practically unknown in Europe until the end of the thirteenth century. At that date numbers were written in Roman numerals. When we see a date inscribed in Roman numerals upon the portal of a public building, we witness the persistence, in art, of the system which was in universal use for all the arithmetical purposes of the civilized world only a few centuries ago.

How simple to our minds to-day seems such a numerical problem as a determination of the number of seconds in a mean solar day. We write down the factors as $24 \times 60 \times 60$, and in about twelve or fifteen seconds, with pencil and paper, we arrive at the answer 86,400. But how forbidding this problem would appear to us in the only form known to our medieval ancestors, namely, XXIV. times LX. times LX. How weary the way that would lead to the answer! Is it any wonder that the counting machine, or abacus, was largely used for the simplest arithmetic; or that the expert arithmeticians in olden days were known under the title of 'sweating calculators'? We read that by the year 1400, the arabic numerals and simple arithmetic were generally known throughout Europe, and were used in most scientific and astronomical works. Most merchants continued, however, to keep their accounts in Roman numerals until about 1550. That is to say, unless history misinforms us, it took about a century and a half for the simple arithmetic of arabic numerals to permeate from scientific circles into the rank and file of the civilized nations. Looking backwards, this seems almost incredible. How was it possible for sensible people to shut their eyes to the simplicity of a scientific rational system of enumeration on the 1 2 3 ... 8 9 0 plan, and adhere to the

laborious, unscientific, stupid system on the I., II., III.... VII., VIII., IX. plan? Nevertheless, sensible people long continued sturdily to resist the innovation. Moreover, it is stated that the change was frequently particularly resisted by the professional arithmetical experts.

The above presentations, while surprising at first apprehension, may claim perhaps a greater admiration and regard for our beautiful, and now universal, arabic arithmetic, which has asserted its supremacy by the laws of evolution and the long struggle for existence from which the fittest and the locally best emerge.

Even after our modern arabic arithmetical system prevailed, it was long before its decimal refinements were reached. According to Ball's history, decimal fractions were only invented about the year 1617, and it was not until the beginning of the eighteenth century that the decimal point came into use. In fact, an examination of eighteenth-century literature seems to indicate that fractions were more generally expressed as vulgar fractions in the earlier part of the eighteenth century, and it was not until the latter part that decimal fractions became customary.

A simple, practical and scientific system of units requires to be a decimal system, in order to transfer from a larger to a smaller denomination, or *vice versa,* by a mere change of the decimal point. This generally acknowledged fact is instanced by our American currency, which has three decimal units—the dollar, cent and mill. In effect, however, there is but one unit, the dollar; because a sum of money expressed in dollars is instantly converted into cents, or into mills, by a shift of the decimal point, without any appreciable mental effort. In Great Britain, however, the currency not being decimal, but divided into guineas, pounds, crowns, florins, shillings, pence and farthings, there are seven different units. Pencil and paper will generally be required by any but skilled mental arithmeticians to transfer a sum of money from one expression to another.

To an American, the superiority of the decimal currency over the non-decimal currency of his British cousin is generally so self-evident as to require no emphasis. But it is notorious that many intelligent and cultivated Englishmen do not recognize this superiority. They are so familiar by habit with their own currency, that they have forgotten their early schoolboy efforts in mastering it. Nevertheless, it is easily shown that the British system, as above enumerated, includes no less than 17 connecting ratios; namely, 1.05, 2, 2.5, 4, 4.2, 10, 10.5, 12, 20, 21, 24, 48, 60, 240, 252, 960, 1,008. The American decimal currency has only three connecting ratios, 10, 100 and 1,000; while these are effected without calculation, by merely shifting the decimal point.

The advantage of the decimal currency could not, however, have been realized before decimal arithmetic became generally known; or, say, prior to the eighteenth century.

What applies to currency units applies also to units of length, area, volume and weight. A simple rational system of such measures must, as is generally admitted, be a decimal system. In this way large and small units may be related like the dollars, cents and mills of our currency. Such a system could not have been introduced until arithmetical science had reached a sufficiently general development, say, at the opening of the eighteenth century.

Commerce and labor must have demanded systems of weight and measures as far back as we have knowledge of the doings of nations. It is no wonder that these systems should have been crude, labor-absorbing and unscientific. No disparagement can be imputed to the English-speaking nations for inheriting from remote ancestry a crude system of weights and measures. Criticism of such nations can surely only be fairly laid at their doors when, seeing that their neighbors have a better modern system, up to date and practical, they remain supine and make no attempt to join the ranks of international progress.

In British and American measures of length we have the following units, all taught in the schools and all used more or less—league, statute mile, furlong, engineer's chain, surveyor's chain, rod-pole-or-perch, yard, foot, engineer's link, span, surveyor's link, hand, inch and line. These numerous units involve more than a hundred cross-connecting ratios, many of which would, it is true, be very seldom called for. Even, however, if we confine ourselves to mile, yard, foot and inch, we have the following six connecting ratios: 3, 12, 36, 1,760, 5,280, 63,360.

In the metric system there is the meter, about ten per cent. longer than the yard, and its decimal derivatives, all evaluated at a glance by a shift of the decimal point. In English speaking countries, roads are measured in miles and furlongs, short distances in yards, houses or ships in feet, horses in hands and small objects in inches. These expressions are not exchangeable or translatable without more or less mental effort. In the metric system, roads are measured in kilometers and hectometers, short distances in meters or dekameters, small objects in centimeters or millimeters and microscopic objects in micrometers or microns. Taking the length of a good-sized bacterium as one micron, it is immediately evident to the mind that a million such bacteria would fit into a meter, and one thousand million into a kilometer. If, however, we take the size of the bacterium as a certain small fraction of an inch, it takes time and considerable mental effort to find the corresponding relation of dimensions.

The same difficulty exists with units of area in the customary system. We have the square inch, square foot, square yard, square rod, rood, acre, square mile and township. All these units are used, although some are used only by surveyors. These involve 45 connect-

ing ratios, some of which are very complicated. In fact, many educated persons have a very imperfect conception of what constitutes an acre, and yet farms or estates are usually valuated and measured in acres. In the metric system, however, the square meter is the basis of all surface measurement and every expression in one unit is convertible into any other unit of the system, greater or smaller, by a shift of the decimal point.

The same comparison and contrast apply to volumes in the two systems. We have the cubic inch, cubic foot, cubic yard and cubic mile. International metric measure has the cubic meter and decimal derivatives. That is to say, there is virtually but one unit.

For the dry measure of volumes, we have, to make our confusion complete, pints, quarts, pecks, bushels, barrels, quarters and chaldrons. Moreover, our bushels, although nominally the same, actually vary according to the commodity measured; there being, according to 'The World Almanac,' some 20 different sorts of bushels by the laws of the United States, from a bushel of bran weighing 20 pounds avoirdupois, to a bushel of fine salt weighing 167. Add to this joy the recollection that there is a difference of about 8 per cent. between the British and American bushels, pecks, quarts, etc., and we attain the happy result that none can say precisely what quantity is meant by the term 'bushel' save by chance or context.

In regard to units of weight, or, more strictly, of mass, the metric system has the gram, which may be defined as the weight of a cubic centimeter of distilled water under specified conditions. Or, expressing the same relation in other words, a cubic meter of water weighs a metric ton of 1,000 kilograms and a cubic decimeter (called a liter) weighs one kilogram. Heavy weights are expressed in metric tons. lesser weights in kilograms, small weights in grams and tiny weights in milligrams. A mosquito will weigh in the neighborhood of a milligram, a U. S. nickel, fresh from the mint, five grams, an average man seventy kilograms, an average elephant about 3,000 kilograms or 3 metric tons. In this manner the weight of any object is brought immediately before the mind in relation to that of any other object, large or small, since virtually one and the same unit—the gram—is used throughout.

In contrast with this simple expression of weights in the metric system, we have in our systems three kinds of weights: viz., apothecary's weight, troy weight and avoirdupois. The two first mentioned have their ounces in common but differ otherwise. Although most substances are sold by avoirdupois weight, the precious metals sell by troy weight, and drugs in prescriptions usually sell by apothecary's weight. The ounce and pound weights kept by the druggists are for this reason generally different from the ounce and pound weights kept in the other

stores. It is impossible, therefore, to determine, except by context, precisely what is meant by the word 'pound' as a unit of weight in English-speaking countries. It may be the troy pound of 5,760 grains, or the avoirdupois pound of 7,000 grains.

By a felicitous arrangement, retained as a relic of the dark ages, pearls and diamonds are still weighed in a system of their own, the carat being 3.2 grains.

The cross-ratios connecting these various units are several hundred in number and very complex. Since, however, the ordinary citizen only deals with avoirdupois weight, in which there are nine units, the cross-ratios are 45 in number. Moreover, there are two tons in this country, a long ton of 2,240 pounds and a short ton of 2,000. In general literature, it is frequently impossible to determine which of these tons is referred to.

When liquid measure is considered, the medley and jumble of British measures is, if possible, worse. The British gallon is defined as 10 pounds avoirdupois of water at 62° F., the volume being 277.274 cubic inches. In the United States, the gallon is fortunate enough to contain 8.3389 pounds avoirdupois, or 58,372 grains, of water at a temperature of 39°.83 Fahrenheit. The U. S. gallon is thus about one sixth smaller than the British. The same happy ratio affects all the subdivisions of each system, viz., pints, quarts, etc. It is often difficult to tell whether English gallons or U. S. gallons are referred to, when the term is encountered in literature. Occasionally an American book will quote British gallons, or *vice versa*, without any reference to the discrepancy. Moreover, we have apothecary's fluid measure containing as units the minim, drachm, ounce and pint. These measures are again different in Great Britain and in the United States.

If we should attempt to collate all the British and American units of volume, both 'dry' and 'fluid,' and express each unit of the table in terms of all the others, the table would contain more than a thousand entries or cross-ratios. In the corresponding table of the metric system there would be virtually only one unit, and all others would be expressed therein by a shift of the decimal point.

If an attempt were made deliberately to construct a medley of weight and measures as a burlesque, for sensible practical people to make sport of, it may be questioned whether such a farcical hypothetical medley would be more illogical, incoherent or cumbersome than our own. Yet it would seem that ours is not worse than the old French, or German, or Austrian, systems that preceded the metric system in those countries, respectively.

It was stated in evidence before the committee on Coinage, Weights and Measures of the House of Representatives in Washington, two years ago, by an expert in education, that about two thirds of a year

of labor in school could be saved to the scholars of all the U. S. public schools, by the substitution of the metric system for the existing system. This saving would enable the scholars to learn more in other directions during the time saved. Nevertheless, it is sometimes gravely asserted that the value of the present system is the difficulty it provides for exchange, and estimates, and computations of all kinds, thus affording useful mental exercise, both for school children and for adults. Fears are occasionally expressed that the substitution of the metric system would make mental arithmetic in such matters so easy that the aptitude would be lost. According to this argument, we should make all mental operations as hard as possible, artificially.

So complex is our customary system of weights and measures, that there are comparatively few persons who can recite from memory all the various tables taught in our schools. So ambiguous is the system, that many cultured persons are not aware of the difference between British and U. S. gallons, quarts, pints, bushels, pecks, etc. Some cultured persons are even unaware of the difference between the apothecary's ounce or pound and the avoirdupois ounce or pound.

The argument is often made that the English-speaking people should adhere, for patriotic reasons, to their national standards as against standards of French creation. Surely the answer to such a plea is that the question is not between the English and the French peoples, but between the English-speaking peoples and the rest of the civilized world. The metric system is the only system of weights and measures that can be called international. Moreover, only the very best available system should be good enough for Americans.

It is sometimes complained that the meter as a unit should be set aside because it is inaccurate. In order to make the standard length international, France decided upon a decimal fraction (the ten millionth part) of the distance between the geographical pole and the equator, measured on the Paris Meridian of the earth's surface. The meter arrived at by the French geodesists at the beginning of the nineteenth century appears by the most recent publications of the Bureau des Longitudes to have been a little short of the mark. It seems that the international meter, defined as the distance between the centers of two marks on the standard meter bar kept in the International Bureau of Weights and Measures at Paris, is just about one-fiftieth part of 1 per cent. shorter than the ten millionth part of the quadrantal arc of the earth above referred to. This small discrepancy is evidently of no material consequence; partly because a discrepancy ceases to be a source of error as soon as its magnitude becomes known, and partly because all copies of the meter are made by bar-to-bar comparison and not from comparison with the dimension of the earth,

which dimension is stated to differ appreciably from meridian to meridian, owing to irregularity of form.

Another objection often made to the metric system is the loss of binary subdivision. In the system of binary subdivision we have halves, quarters, eighths, sixteenths, thirty-seconds and sixty-fourths, etc. It is contended that in decimal division these subdivisions become awkward beyond halves; viz., 0.5, 0.25, 0.125, 0.0625, 0.03125, 0.015625, etc. This is, no doubt, a weak point in the decimal system generally. If the base of our notation were 12, or 16, instead of 10, the objection would be made more remote. But it is useless at this epoch to discuss an international change of arithmetical notation. There does not seem to be the least prospect of such a change, nor the least hope of its being made in the near future. Moreover, the same objection applies to our decimal currency, and is scarcely felt in that direction. Brokers reckon in the binary scale to one eighth, but are said not to employ sixteenths. An inch is often subdivided to sixteenths, but thirty-seconds are seldom used, sixty-fourths very rarely, and yet smaller binary subdivisions are almost unknown. In fact, where fine micrometer measurements are made in inch measure, they are nearly always in decimals of an inch, and not in binary subdivisions. In metric countries, the decimal subdivisions do not seem to constitute a noticeable hardship.

Most persons grant that the metric system is superior for practical as well as scientific purposes to the British system, but dread the cost of a change or transition. There can be no doubt that the question of expense of transition is a serious one. In fact, if the only alternatives were the immediate compulsory adoption of the metric system on the one hand, to the extent of throwing away every existing measure and standard, or never adopting the metric system, on the other hand, it is probable that the latter alternative would be necessary; for the trouble, vexation, expense and litigation to be expected from immediate change would be terrible to contemplate. Fortunately, no such alternatives are presented. We have the history of almost all the continental nations of Europe to guide us in estimating the degree of difficulty which would be expected in effecting the change.

In France, the native land of the metric system, and the first country officially to adopt it, the change was made very slowly. During the first half of the nineteenth century France stood almost alone in this reform. Moreover, the initiation of the reform in 1795 took place in the year III. of the French Republic, and was doubtless greatly aided by the general upheaval of long established customs and traditions in France about that time. If it had not been for the French Revolution, so terrible in many of its aspects, the metric system might never have become a practical reality.

In the latter half of the nineteenth century the reform spread over continental Europe. Spain officially adopted the system in 1849, Italy in about 1850, Portugal in 1852, Switzerland virtually in 1851, through the medium of a transition system, and, finally, in 1877, when the complete system was officially adopted. Germany and Austria-Hungary officially adopted the system in 1871. Russia, semi-officially, in 1900. Denmark alone in continental Europe has declined to make the change; but even there, it is said that the system is much used, owing to the influence of neighboring countries, in spite of the government attitude.

The pressure upon the continental European nations to adopt a uniform international system has doubtless been considerable, owing to their relatively close geographical association. Beyond the advantages inherent to the system and its international use, there does not seem to have been any pressure which would have brought about the change. Few persons now living in Spain or in Italy would be able to remember the conditions at the time of inaugurating the metric system. In Germany, however, the official change was made by law only about thirty years ago, and the events connected with the change are remembered by a large section of the people. The testimony seems to be that the change was virtually made in the cities during the course of a few weeks, and in the country districts during the course of a few months; so that in a year the metric system was practically universal. The manufacturers continued to use their tools, standards and machines just as in the past; except that they gradually measured their products in the new units; and as time went on, and machines became renewed, the machines were changed in such a manner as to produce even metric sizes.

So far as can be ascertained from the history of transition to the metric system abroad, the transition in this country should not require any machine, tool or piece of apparatus to be discarded or abandoned. The difficulty of transition would not be in expensive new machinery. It would lie in translating the old familiar sizes made by existing machinery into the new units. The trouble would be intellectual rather than material. New price-lists would have to be prepared in terms of the new units. In stores where sales had been previously made by the yard, they would be made, in the new regime, by the meter, which is a measure about ten per cent. longer. In stores where sales had been made by the pound, the new sales would be by the kilogram, which is somewhat more than two pounds (about 2.2). This would involve a change of foot-rules, yard-measures, and sets of scale-weights, together with a change of price-lists. If the change occurred suddenly, there would be great confusion; but if it took place gradually, the trouble would probably not be serious. To

a person accustomed to buy sugar at six cents a pound, it would be a little perplexing at first to buy it at 13 cents the kilo, or 6½ cents the half-kilo. It is the experience of the average American, however, that in a foreign city, where the metric units and coinage have both to be acquired, familiarity is obtained after about two weeks. Consequently, less time should be needed to gain familiarity with the metric system, when the coinage is unchanged.

If it be questioned as to whether it is worth while for the whole nation to be involved in this trouble and expense of transition, it should be remembered that practically all countries, except the English-speaking countries, have already considered it worth while to make the change, and that none of these countries has expressed regret for the step. Moreover, the labor involved in the change, if the transition is not too sudden, will be small compared with the labor saved to the young in acquiring the present complex system, as well as to adults in wastefully consuming time for constantly applying it.

The United States cover so large a territory, and have within this territory so large a market, that the pressure upon their citizens for meeting the requirements of trade with countries outside of Great Britain, or her colonies, has not been felt as it has been felt in countries like Germany. Nevertheless it is the often-repeated statement of the U. S. consuls living abroad that the non-metric price-list, weights and measures of American manufacturers are a handicap on American trade with metric countries.

Already the metric system is used in the United States for nearly all scientific work and literature. It has even permeated popular literature to some slight extent. It has already invaded pharmacy and microscopy. The international electric units are metric units. It has come into our currency. A nickel weighs, by law, five grams, and a dollar twenty-five. The U. S. foot is defined by law as a certain fraction of the international meter.

The colonies of Great Britain, less conservative than their mother country, have recently urged Great Britain to adopt the metric system. In August, 1902, the prime ministers of the colonies officially urged upon the Secretary of State the importance of adopting the metric system throughout the British Empire. At the Congress of the Chambers of Commerce of the British Empire, held in Montreal in August 1903, with delegates from all parts of the empire, a strong resolution was adopted concurring with this action of the colonial prime ministers, and urging the British government to adopt the metric system.

The parliament of Australia passed a strong resolution in favor of adopting the metric system throughout the British Empire, in June, 1903. A similar, but less decided, motion was carried in the Cape of

Good Hope House of Assembly in August, 1903. A similar action was taken about the same time in the New Zealand parliament.

A bill for the compulsory adoption of the metric system in Great Britain within a specified period, passed the House of Lords last year, and has also passed its first reading in the House of Commons. The bill was supported by petitions from councils of cities, towns and counties, having a total population of 2,800,000; as well as by trade-unions and other organizations to the total enrolment of one third of a million persons.

In this country, a bill for the compulsory adoption of the metric system within a specified time was introduced into congress in 1903, but was withdrawn. Resolutions in favor of the adoption of the metric system in the United States have been passed by the Franklin Institute, the American Institute of Electrical Engineers, and the American Electrochemical Society. On the other hand, resolutions opposing the adoption of the metric system have been recently passed by the American Society of Mechanical Engineers, the American Association of Toolmakers, and the Association of American Manufacturers. The principal objection in these cases seems to have been the dread of expense in transition.

It would seem to be only a question of time when the elimination of the useless, and the survival of the fittest, will bring about the universal adoption of the system. Even assuming, however, that the change were made officially by the United States government within the next ten years, the existing units would continue to persist in some degree for many years. Thus inch-pipes will doubtless exist in the country for many years to come as a physical reality; even though such pipe should come to be called 25-millimeter pipe. Even at this time, more than one hundred years after the inauguration of our decimal currency, one still occasionally hears the 'shilling' quoted as a price, a relic of colonial currency. When so used in New England, a 'shilling' appears to mean one sixth of a dollar, or 16 2-3 cents.

In time to come, and probably much beyond the date of the universal adoption of the metric system, decimal reform may perhaps extend to other fields. Thus the cumbersome and complex systems of dividing angles sexagesimally into degrees, minutes and seconds, is generally admitted to be much inferior to a decimal subdivision. Some day, perhaps, angles may be expressed decimally all over the world. The day also is divided in a cumbersome way into hours, minutes and seconds. A decimal subdivision of a day would have much advantage over the existing plan. But decimal reform in angles and in time is undoubtedly much more remote and problematical than in weights and measures; nor is there the same exigency for decimal reform in these directions.

A BOTANICAL LABORATORY IN THE DESERT.

By Professor FRANCIS E. LLOYD,
TEACHERS COLLEGE, COLUMBIA UNIVERSITY.

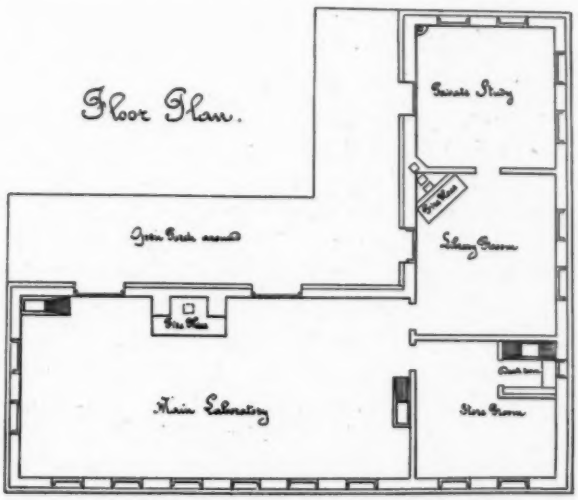
TWO years ago the Carnegie Institution determined to establish a laboratory to be devoted to the special study of desert vegetation. The plan originated with Mr. Frederick V. Coville, who, under the auspices of the Smithsonian Institution, had for some dozen years previously been a close student of the plants of the southwestern American desert. His interest was fixed by his experiences as a member of the memorable Death Valley Expedition of 1891. After Mr. Coville's plan had been adopted by the Carnegie Institution, an advisory board, consisting of Mr. Coville and Dr. D. T. MacDougal, was appointed. The first work of this board was the choice of a proper site—a task which will be conceded to be neither easy nor unimportant when the great extent and variety of the North American Desert is appreciated. Both of these gentlemen were, however, possessed of wide personal knowledge and experience of this region and brought to the solution of the problem ripe judgment. After a further personal examination of all of the most promising areas, including the deserts of Texas, northern Chihuahua and Sonora in Mexico, New Mexico, Arizona and of California, the choice rested upon Tucson, in southern Arizona. The results of this investigation are embodied in an extensive report* which is full of valuable data and most instructive and beautiful illustrations. The wisdom of the choice of the advisory board may very naturally be questioned, and I confess to have entertained some doubt in this regard. After a personal examination, however, of nearly all the above mentioned regions, and after spending the major portion of the past summer at the Desert Botanical Laboratory, I am now of the opinion that the action was well-advised and is fully justified. I am therefore taking occasion at this time to give an account of the laboratory and its surroundings from my own point of view.

The city of Tucson, with a population of 10,000, is situated in the valley of the Santa Cruz. Its position is central with respect to the deserts of California, Mexico, Texas, New Mexico and northern Arizona. With an elevation of 2,390 feet above sea level, it has a hot, though dry and bracing, climate. The soil is a fine clay or adobe, underlaid by a white hard pan, locally known as caliche. Two miles

* 'Desert Botanical Laboratory of the Carnegie Institution,' Publication No. 6, November, 1903.

THE DESERT LABORATORY, LOOKING NORTH.

to the westward are to be seen the outposts of the Tucson Mountains, rugged hills of volcanic origin. On the more gradual northerly face of one of these, on a shoulder of gentle slope, stands the laboratory, a building appropriately constructed of the volcanic rock. The style is simple and well adapted to the climate. The thick stone walls heat slowly, particularly as they are for the most part protected from the direct rays of the sunlight by an overhanging roof. This latter is so constructed as to form a large ventilated air chamber, itself a protection from the effect of intense insolation as well as affording comfort to the occupants by modifying the strong light.



The form of the building is in the shape of the letter L, with the longer outer elevation facing the north. The dimension and disposition of the space within are indicated upon the accompanying plan. The windows are abundant, and when open to the brisk winds allow a current of air throughout the building which makes one forget the heat. About this factor much misapprehension is abroad. To be sure, the mercury stands high, and a registration of 100–105°F. is not at all unusual; occasionally it is even higher, and doubtless if one did not forget to look at the instrument, still higher temperature could be discovered. Fortunately, this is just what one does. There is so little discomfort attendant upon the heat that it is usually quite disregarded, a condition happily due to the low relative humidity, which on many occasions during six weeks in July and August of this year ran down as low as 7 per cent. The only feature of discomfort to some persons is the intense illumination, and one may require dark glasses, although I personally found no discomfort even upon the mesa. On

A Corner of the Laboratory. A Palo Verde on the Right.

the volcanic hills, the dark color of the ground affords very appreciable relief.

The general laboratory is furnished with individual tables, a general work table and two sources of water supply. It should be noted that, although two miles from town and upon an elevation of 300 feet above it, there is abundant water supply. It is to the enlightened interest of the citizens of Tucson that this convenience, as well as the electrical connections, acreage and roadways, are to be credited. The immediate source of water is a 300-gallon tank, supplied from a pumping station at the foot of Sentinel Hill, which stands immediately to the southeast of the laboratory.

THE GENERAL LABORATORY.

Adjoining the general laboratory is the stock room, liberally supplied with the necessities for work. This leads into a photographic dark room, with water supply, ventilation and a cement floor, and which may be used as a physiological dark room, with constant temperature.

The library, which adjoins the stock room, though at present small, contains a carefully selected lot of periodicals and books, the latter chosen with special reference to their bearing upon desert exploration and vegetation. From the library one enters the office of the resident officer, Dr. W. A. Cannon, whose generous treatment and constant sympathy, coupled with the material opportunities afforded, leave little to be desired.

The view commanded from the laboratory site is a panorama of

rare beauty. To the north lies the range of the Santa Catalina Mountains, extending through 40° of the horizon. Its rugged topography scarcely noticeable in the glare of the high sun, is thrown into bold relief when the shadows begin to lengthen. Then the dazzling purples and yellows of midday give way to the deep blues and purples of the valleys contrasted with the reddening tones of the higher slopes and ridges. The well-wooded and watered regions of this fine country are within two days' travel of Tucson, and excursions may be made thither with comparative ease.

To the east stands the rounded mass of the Rincon Mountains, the illumination of which by the afternoon sun is most remarkable. The vividness of details, the shimmer of heat, the blaze of reflected light modified by the merest veil of purple—these are faint expressions of what one quite fails to describe. To the west the desolate defiles of the Tucson Mountain, seen at short range, gives us, with the sun in the same position, a contrast picture of hard and rugged profile, dark browns and black shadows. To complete the panorama, one needs but to climb to the top of the hill, from which may be seen in

PRICKLY PEAR.

the far distance the deserts of Sonora and the malpais—the rendezvous of the few 'bad men,' now so hard to find.

Aside from the conditions for study offered by the Desert Laboratory as such, the matter with which the student is especially concerned is the plant life. In seeking for the right place to plant a laboratory for the study of desert vegetation, it is obvious that some practical conception of what such a vegetation is had to be formulated by the advisory board. It was necessary for this board to find a locality with a desert climate and possessed of as rich and varied a flora as possible,

while still of a distinctly desert character. Since it is the chief object of the laboratory to study 'drought-resistant vegetation,' it would have been absurd to put the laboratory in an out-and-out desert, and but little better to have selected a semi-arid region with a rich flora. Nor would it have been foresighted to have chosen a locality which might sooner or later be threatened by irrigation. The conditions above stated may, of course, be met in many places, but scarcely better than on the hills west of Tucson, and on the adjacent slope and mesa. The general character of the vegetation here is in the main similar to that of the mesa and rocky ridge of the whole territory between Texas and western Arizona, but is, also, within the limits of distribution of the saguaro or giant cactus (*Cereus giganteus*). It is, therefore, representative in this important respect, of a very wide stretch of country which is of an undoubtedly arid character, the plants of which are, with the supply of water derived from a meager rainfall and a little snow, able through long periods of drought to sustain their powers of growth unimpaired. A more immediate view of a few of the more striking of these desert plants will here be of interest.

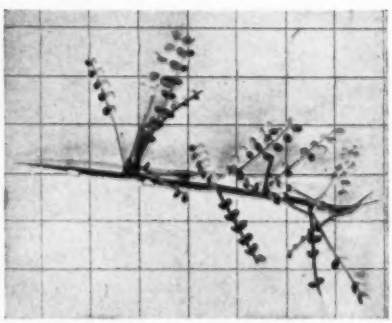
Ephedra.

The vegetation of the country about Tucson is very naturally and obviously divided into two formations which occupy, the one the level mesa, the other the rocky hills and slopes. We may consider first the mesa, where the most prominent element is the greasewood, or creosote bush (*Covillea tridentata*), a plant of rather singular aspect. The average plant in point of size stands two and a half to three feet in height. The numerous branches, which arise together from the soil, spread radially at an angle with its surface, and bear in their upper parts a meager foliage which, in times of drought, is olive-brown in color. The leaves, the form of which is peculiar to the family

(Zygophyllaceæ) of which the plant is a representative, are coated with appressed hairs and varnished with a resinous secretion. After the advent of a rain, or if watered, the plant quickly responds, the leaves becoming bright green and its delicate yellow flowers coming out in great numbers. The shrub is flat topped, and in large areas where few other so large specimens grow, the foliage of many plants lies in a uniform layer parallel to the ground. The hard twigs and branches make excellent fuel particularly for the camp cook's fire.

YELLOW-FLOWERED OPUNTIA.

Associated with the creosote bush, in numbers varying with the locality, are several species of cacti of the genus *Opuntia,* but of the type of *Opuntia arborescens,* and not possessed of the flattened branches of the common prickly pear. Of these, which are more dense tree-like in form, the one known locally as the cholla* attracts one's attention most quickly. Its smaller branches, clothed with numerous white spines, are curiously massed into formless bunches many of the smaller joints hanging down and sooner or later breaking off. Thus collect beneath the plant piles of *dejecta membra,* many dead, some dying. These broken off parts may serve to propagate the plant; when seed-pods are so cast off they more likely serve this end. In the agglomerations of branches just described birds cunningly build their nests; and these are hardly to be noticed but for the knowledge that they are frequently found in such places. The few stout ungainly stems of these plants, and, as one may say, the weird disposition of the lesser branches, glistening in their coating of gray shining spines, produce a habit which, in the language of the advertisement, is peculiar to itself. When, as often happens, the cholla has the monopoly of the situation, the desert aspect becomes very pronounced. Two other species of this type are common, the so-called *Opuntia spinosior,* with pink flowers, and another (*O. versicolor*), with yellow ones. These, however, do not show



TWIG OF PALO VERDE.

* Pronounced Cho-ya (*Opuntia fulgida*).

the peculiar massing of branches, nor are their ultimate articles so readily detached. Perhaps more interesting than all these is the little half-viny *Opuntia leptocaulis,* with stems, of scarcely the thickness of a lead pencil, interweaving among the branches of some companion plant, usually the creosote bush. It is never to my knowledge found growing except in the immediate proximity of another plant, and thus often escapes detection. It is not very well supplied with spines, and might easily be destroyed by trampling if away from the company of a plant of sturdier growth. Perhaps this is the reason why it usually is so found—it is stamped out elsewhere. Or possibly it needs the partial shade afforded it. This is a question worth an answer.

If you travel for a distance in any direction, other plants will be found. Among these are sweet-smelling acacias, ungraciously, but, I fear, not undeservedly, called cats-claws, with their finely divided leaves and small yellow pompons of flowers. The low leafless shrub *Ephedra,* with its vertical green stems which look like the scouring rush and is as rough and hard to the touch, is another—a typical desert plant if ever there was one. This plant is a relative of our yew, but is possessed of very unique characters, the description of which would take us too far into details.

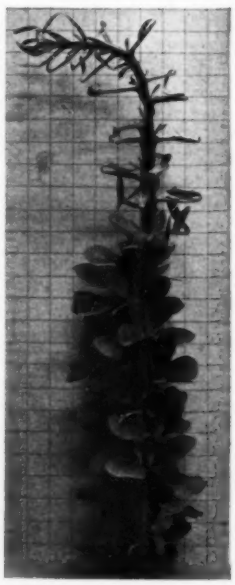
Here and there a 'salt' or 'alkali' spot is to be found. Here grow few enough plants, and these such as can endure the hardships of a most unfavorable soil, as for example *Atriplex* and *Dondia.* Near the water courses, from which for the greater part of the year water is conspicuously absent, one finds, on the other hand, larger shrubs and very small trees of *Acacia,* mesquite (*Prosopis velutina*) and a species of palo verde (*Parkinsonia*). Ten miles south of Tucson, near to the mission of San Xavier del Bac, the river bottom is occupied by a veritable forest composed wholly of large mesquite, and this the Papago Indians of the region draw upon chiefly for wood. I should mention the presence in parts of the mesa of the many low shrubs which are noticeable chiefly for their inhospitable thorniness. The palo christi (*Koeberlinia*) is an extreme type. For a crown of thorns no better material could be imagined than this.

On approaching the rocky slopes leading to the higher elevations, a different vegetation is met—different in species, but not in general character. At closer range a distinctively green note in the coloring is appreciated, which at a greater distance was a uniform brown. It is the palo verde (*Parkinsonia microphylla*), so called in the Spanish on account of the uniform green color of all its members, which gives the impression of verdure. This is a small, somewhat gnarled tree, related to our locust, of the general appearance of a trimmed orchard tree. It is usually leafless, although the younger shoots are sometimes sup-

plied with a meager foliage of compound leaves. The very small leaflets, which are foliar members of the third order, are sensitive, the pairs folding together at night and opening at dawn. Closure again occurs at the period of excessive insolation. All the ultimate branchlets taper into long stiff thorns, an example of direct metamorphosis, since they originate as normal shoots. In the absence of leaves it is evident that the chlorophyll tissues of the stems are chiefly concerned in the food-making process, which would appear true also from the fact that the smaller branches and twigs are used as green forage for horses in winter. Although a particular branch removed from the tree and regarded alone is a rather graceless object, the whole tree with its smooth green bark and gradually tapering limbs and twigs is singularly beautiful, its outline most delicate.

OCOTILLO.

Conspicuous as a member of the hillside vegetation is the ocotillo (*Fouquieria splendens*), a plant with a hypothetical relationship with the willows, but not in the least suggesting them by its habit and more obvious structures. The general disposition of its branches, which are highly suggestive of coach whips, is similar to that in the creosote bush. It is, however, a much taller shrub, with lithe, bespurred stems, bearing in spring each a brilliant mass of scarlet flowers. On the advent of the rains, the stems are quickly and completely clothed with rosettes of light green ovate leaves, each rosette in the axil of a thorn. Most interesting is the manner in which the thorns arise. The new shoots produce first the primary leaves, in the stalks of which a hard tissue is developed. Their leaf-blades rather soon wither away, and split away from the harder part of the stalks, which in this way are left as spines, in the axils of which, as above stated, the second-



TWIG OF OCOTILLO.

VIEW ON THE LABORATORY HILL, GIANT CACTI.

ary leaves develop. The bark of the ocotillo is waxy and burns like a candle.

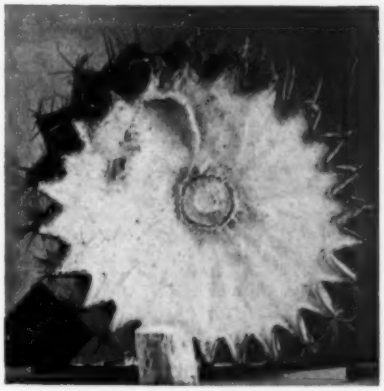
But whatever of unusual beauty or peculiarity the palo verde or ocotillo possess, the plant which of all claims and holds the attention the most constantly is the great sentinel-like saguaro or giant cactus (*Cereus giganteus*). Until middle age it is a single green fluted column, with a nature-wrought entasis, a form of severe simplicity scarcely less emphasized when, in its later growth, a few arms grow out. These are usually found at some distance below the middle point of the main shaft, and, though for the most part erect, sometimes, partly by accident, take most grotesque positions. The woody skeleton is not extensive, consisting of slender ribs which occasionally anastomose. These are used by the Papagos for palings and to form the frame of their burden frame or quijo.

The saguaro flowers in June, producing at the top of the stem a large cluster of tubular white petaled flowers in which are found hundreds of small beetles and wasps, apparently the chief agents of pollination. While the fruit is developing, the shriveled flowers, which re-

SAGUARO FLOWER AND FRUIT CUT LONGITUDINALLY.

LOOKING NORTH FROM THE DESERT LABORATORY.

main for some time attached, lend a shaggy top-knot appearance which for the while detracts from the dignity of the plant. When the fruits are ripe, the fleshy pericarps split, disclosing the crimson pulps studded with small, black seeds. The effect, at this time, if one stands at a modest range suggests a mass of crimson flowers. The fruits are sought for by Mexicanos and Papagos with an



BARREL CACTUS CUT ACROSS.

BARREL CACTUS.

avidity which to my own taste is scarcely justified by their flavor, that of a ripening fig.

Another prominent member of the cacti is the barrel-cactus (*Echinocactus Wislezeni*), which is two feet in height and eighteen inches in diameter, and is armed with large hooked spines. In this plant, as will be seen in the illustration, the rind is remarkably developed, this tissue containing about eighty per cent. water. As the sap when extracted is potable, this species, as well as certain others, is used as a source of water to quench the thirst. The prickly pears are found in profusion, and represent at least two species, one a shrubby form, four feet in height, the other a sprawler. The fruits of these two plants are also collected and eaten.

Smallest among these curious plants is a species of *Mamillaria*, each protuberance of which is surmounted by a radiating group of

MAMMILLARIA.

ENCELIA.

delicate spines. From the center of each group arises a black slender hooked spine. The flower is a relatively large one of a dainty rose color.

Space does not allow further description and the remaining shrubby vegetation, including the *Lycia* and *Celtis,* must be passed with mere reference. Two weeks after the advent of the rains the ground is clothed with many richly colored and often fragrant annuals and small perennials. Some of the latter, as for example an *Encelia* and a *Cassia,* persist through the drought, a hardiness explained in part, at least, by the felt-like protective layers on the leaves. Among the less resistant, but rapidly growing herbs are the fragrant flowered *Martynia,* with its large, double-hooked pods, and a *Tribulus,* bearing rich yellow poppy-like flowers.

With such surroundings, rich in material and opportunity, what may be accomplished at the desert laboratory? Much of the taxonomic work of this region has been done; but, in the light of modern research, much remains to do. The structure and development of scarcely one of these desert forms is properly understood, and a wide field awaits the student in these directions, while the peculiar physiology of these plants has scarcely been touched upon. Physiological and anatomical-physiological studies of wide extent may here be carried on. It is to the honor of American science that an opportunity such as this has been afforded by the Carnegie Institution.

GALILEO.

II.

By Dr. EDWARD S. HOLDEN,
U. S. MILITARY ACADEMY, WEST POINT, N. Y.

Galileo in the *Sidereus Nuncius* (1610) gives this account of the invention of the telescope:

A report reached my ears that a Dutchman had constructed a telescope, by the aid of which visible objects, although at a great distance from the observer, were seen distinctly as if near. . . . A few days after, I received confirmation of the report in a letter . . . which finally determined me to inquire into the principle of the telescope* and then to consider the means by which I might compass the invention of a similar instrument, which, in a little while, I succeeded in doing, through deep study of the theory of refraction. . . . At length I succeeded in constructing for myself an instrument so superior that objects seen through it appeared . . . more than thirty times nearer than if viewed by the natural powers of sight alone.

On the title page of his book the telescope is described as 'lately invented by him.' This claim Galileo does not make, but in subsequent years it was charged by his enemies that he claimed credit not his due, and the charge perpetually reappears. The amazing discoveries of this memorable year are enumerated on the title page in question.

The Sidereal Messinger (Nuncius Sidereus), unfolding great and very wonderful spectacles and offering them to the consideration of every one, especially of philosophers and astronomers; being such as have been observed by Galileo Galilei . . . by the assistance of a perspective glass lately invented by him; namely, in the face of the Moon, an innumerable number of fixed stars, the Milky Way, and nebulous stars, but especially respecting four Planets that revolve about Jupiter at different intervals and periods with a wonderful celerity; which, hitherto not known to any one, the author has recently been the first to discover and has decreed to call the Medicean Stars. (Venice, 1610.)

The surface of the moon was covered with brilliant and dark areas as the peacock's tail with spots. Perhaps the moon has an atmosphere, he says. The heights of lunar mountains can be fixed by measuring their shadows. The ashy-light of the moon ('old moon in the new moon's arms') is perhaps caused by a lunar twilight. He gives Leonardo da Vinci's explanation also—the true one—that it is caused by earth-light reflected to the moon and back to us. The stars appear as points of light, the planets as small discs. The telescope brings count-

* Galileo uses the words perspicillum, occhiale, etc., for the instrument. The word telescope was invented to describe the new instrument by Demiscianus at the request of Prince Cesi, president of the Accademia dei Lincei about 1612. The telescope itself was invented by Hans Lippershey.

less new stars to light. In the belt and sword of Orion he sees eighty stars where only seven were known before; in the Pleiades forty instead of six or seven. The Milky Way is a multitude of faint stars clustered together. The nebulæ of Orion and Praesepe are formed of stars. His discovery of the moons of Jupiter dates from January 7, 1610, when three of them were seen. They describe circular orbits about their planet. Jupiter, like each of the planets, has an atmosphere, he says. His telescope was not perfect enough to show this. It is a deduction from analogy.

New discoveries soon followed in respect to Saturn (Dec., 1610) and Venus (Jan., 1611), and they were announced in anagrams as follows:

SMAISMRMILMEPOETALEVMIBVNENVGTTAVIRAS.
ALTISSIMVM PLANETAM TERGEMINVM OBSERVAVI.
(I have observed the highest planet—Saturn—to be tri-form.)
HÆC IMMATURA À ME JAM FRUSTRA LEGUNTUR. O. Y.
CYNTHIA FIGURAS AEMULATUR MATER AMORUM.
(The mother of the loves—Venus—emulates the figure of Cynthia—the Moon.)

The latter discovery was of capital importance. If the planet Venus revolved about the sun as Copernicus had said, it must show phases like the moon. The phases, invisible to Copernicus, were revealed by the telescope. They occurred at the precise times required to demonstrate the truth of his theory. It was now no longer a theory. It was proved. From this moment no competent witness could doubt the truth of the Copernican system—Galileo less than any one.

An opportunity unique in the history of the world was presented to Galileo and he utilized it to the full. He went from triumph to triumph. The phases of Venus, the mountains of the moon, the constitution of the Milky Way, the tricorporate figure of Saturn, the solar spots, the moons of Jupiter, were death-blows to the systems of Aristotle and of Ptolemy, and were skilfully utilized to establish the system of Copernicus. That system rests, for us, not on the telescopic discoveries of Galileo, but on the working out of its details by Kepler and Newton. To the Italians of Galileo's day Kepler was all but unknown; it is even doubtful whether Galileo appreciated Kepler's splendid discoveries; it is, at any rate, certain that he never publicly mentioned them with praise.

The mere fact that the number of planets and satellites was increased by Galileo's telescope from seven to eleven was another blow to ancient superstitions. Seven was a mystic and magical number. It had relations even to Christianity, so his contemporaries thought. The seven golden candlesticks of Revelations were the seven planets. We can form some idea of the hold of certain magical numbers on the imaginations of our ancestors by remembering that when Huyghens dis-

covered a satellite to Saturn—thus raising the number of celestial bodies to twelve—he looked no more, 'because twelve was universally admitted to be a perfect number.' There were six planets and six satellites and he ventured to predict that no more would be discovered. Huyghens died in the year 1695. He was the foremost man of science on the continent of Europe.

In 1610 Galileo had seen Saturn 'tricorporate'—in December, 1612, he writes:

> Looking on Saturn I found it solitary without the assistance of its accustomed stars and, in short, perfectly round and defined like Jupiter; and such it still remains. Now, what can be said of so strange a metamorphosis? Are the two smaller stars consumed like the spots on the Sun? Have they suddenly vanished and fled? or has Saturn devoured his own children? or was the appearance indeed fraud and illusion, with which the glasses have for so long time mocked me, and so many others who have so often observed with me? Now, perhaps, the time is come to revive the withering hopes of those who, guided by more profound contemplations, have followed all the fallacies of the new observations, and recognized their impossibilities. I cannot resolve what to say in a juncture so strange, so new and so unexpected. The shortness of the time, the unexampled occurrence, the weakness of my intellect, and the terror of being mistaken, have greatly confounded me.

The explanation of the disappearance of the ansæ of Saturn's ring was not given until 1656 (by Huyghens). Galileo's telescope was not sufficiently perfect and he died without solving what was a mere riddle to him.

The spots on the sun were first seen by Galileo, though they were first described by others (Fabritius, Scheiner). In April, 1611, Galileo exhibited them at Rome to an audience of notabilities. His own observations had convinced him, he says, that the spots were real; that they were not fixed at one part of the solar globe; that they had motions; he sees no reason to doubt that they are attached to the surface of the sun; he believes that they form at the sun's surface, are dissipated and may reappear. By August, 1612, he made other observations which confirmed his earlier conjectures. Their motions prove that the sun is spherical and that it turns on an axis. He notes also that the spots all lie within certain special zones of latitude. He observes the sun by projection—by receiving its image on a sheet of cardboard. Certain large spots can be seen by the naked eye, but by an inveterate prejudice that the heavenly bodies are incorruptible, they have not been remarked; to the shame of astronomers, he says, such appearances have previously been taken for Mercury in transit over the solar disc.

Galileo's discoveries were received with incredulity by the wisest men of Italy. The warm-hearted Kepler (April, 1610) was the first to recognize 'the divinity of his genius.' Little by little they made

their way as Galileo demonstrated them triumphantly to friends and enemies. Arguments of all sorts were brought against them and against the heliocentric theory which they supported.

Animals, which move, have limbs and muscles; the earth has no limbs and muscles, therefore it does not move. It is angels who make Saturn, Jupiter, the sun, etc., turn round. If the earth revolved, it must, also, have an angel in the center to set it in motion; but only devils live there; it would therefore be a devil who would impart motion to the earth. . . . (Scipio Chiaramonti.)

Since it can be certainly gathered from Scripture that the heavens move above the earth, and since a circular motion requires something fixed around which to move . . . the earth is at the center of the universe. (Polocco, 1644.)

If the earth is a planet, and only one among several planets, it can not be that any such great things have been done especially for it as the Christian doctrine teaches. If there are other planets, since God makes nothing in vain, they must be inhabited; but how can their inhabitants be descended from Adam? How can they trace back their origin to Noah's ark? How can they have been redeemed by the Savior?

The last paragraph is probably an answer to Galileo's opinion (December, 1612) that the moon and planets may be inhabited, though by creatures different from ourselves. Galileo writes to Kepler (August, 1610):

You are the first and almost the only person who . . . has given entire credence to my statements. . . . We will not trouble ourselves about the abuse of the multitude. . . . In Pisa, Florence, Bologna, Venice and Padua many have seen the planets; but all are silent on the subject and undecided. . . . What is to be done? . . . I think, my Kepler, we will laugh at the extraordinary stupidity of the multitude. What do you say to the leading philosophers of the faculty here, to whom I have offered a thousand times to show my studies, but who . . . have never consented to look at planets, nor Moon, nor telescope? Verily, just as serpents close their ears, so do these men close their eyes to the light of truth. . . . People of this sort think that philosophy is a kind of book like the Aeneid or the Odyssey and that the truth is to be sought, not in the universe, not in nature, but (I use their own words) by comparing texts! How you would laugh, he goes on, if you heard the first philosopher of Pisa trying to 'argue the new planets out of heaven.'

While Galileo was teaching the elements of Euclid at Padua his colleague, Cremonini, was expounding Aristotle's *de Cælo*. It was Cremonini who refused to look at the newly discovered satellites of Jupiter through the telescope, alleging as a reason that their existence was quite contrary to Aristotle's philosophy. It was the same Cremonini who, in 1619, with a dignity and firmness that must be sincerely admired, flatly refused to change the substance of his university lectures at the demand of the Grand Inquisitor of Padua. His duty was to expound the words of Aristotle as he found them, he said; he declined to teach as Aristotle's any doctrine that he did not sincerely believe to be the master's. Let this manly stand be counted off against his refusal to be convinced against authority. He is reputed to have

been the last scholastic. When he died, in 1631, there was no one to take his place. The times had changed. We are accustomed to attribute all the merit of the change to Galileo, whose career so brilliantly represents what was best in the new scientific spirit. It is impossible to declare what the movement of the world would have been had Galileo never lived. It would, perhaps, have been much the same. A company of less brilliant men would, perhaps, have done Galileo's work, taking a century for the task. Scholasticism was already moribund; the telescope was invented; the time was ripe; Kepler had already discovered his great laws of planetary motion; who can doubt that scholars would have arisen to fill the opening opportunity?

Gradually the fame of Galileo rose to a great height. He became the best known man in Europe. His lecture rooms were crowded. At Easter, 1610, he showed the Medicean stars to Cosmo II in Florence, and in May he writes a letter describing the work that he has projected—treatises on the constitution of the world, on mechanical motion, on sound, color, vision, tides, fortification, tactics, artillery, sieges, surveying, etc.* This letter soon brought an offer from the Grand Duke to appoint Galileo first philosopher and mathematician at the University of Pisa at a salary of 1,000 scudi. He is not to be obliged to reside at Pisa—and in fact his duties were usually performed by substitutes.

In July, 1610, Galileo left the service of Venice for that of Florence. It was a sad exchange for him. Venice was the only state in Italy that dared to stand up against the power of Rome. There were weighty reasons of state why the Duke of Florence could not do so. The Jesuits had been banished from the soil of Venice (1606) 'for ever.' They were all powerful in Rome and in Florence. It is evident from letters of this time that Galileo's desertion of Padua produced an unfavorable impression of self-seeking even among his friends.

Galileo's visit to Rome in March, 1611, was a veritable triumph for him. His expenses were paid by the court, he was lodged with the Tuscan ambassador, and received with the greatest honor by the Pope (Paul V.) and the cardinals, including Cardinal Barberini, the future Pope Urban VIII. To them he showed his discoveries. They were convinced and interested. At the request of Cardinal Robert Bellarmine, four learned men of the Roman College (Clavius among them) reported on what they had seen through the telescope and fully confirmed his observations. This report is of great importance, since it was, in effect, a sanction by the Church itself. Galileo was received a member of the *Accademia dei Lincei*, and its president, Prince Cesi, became his lifelong friend. The Cardinal del Monte writes to the

* Compare the letter of Leonardo da Vinci to the Duke of Milan reciting the labors that he was ready to undertake in his service.

Grand Duke of Florence (May 31, 1611) that Galileo had given great satisfaction: 'Were we still living under the ancient republic of Rome I verily believe there would have been a column on the capitol erected in his honor.' Galileo was at the top of the wave of fortune to all appearance. At this very moment, however, Cremonini's trial was going on before the Roman Inquisition and on the records is an inquiry whether Cremonini and Galileo were in any relation with each other. He was already suspected of heresy. His friendship would, even then, have been prejudicial. By 1613 Galileo was aware that there was a league of his Florentine enemies against him. In a letter to Prince Cesi he makes light of it. 'I laugh at it,' he says, but it was none the less serious. It was based on religious scruples, but stirred to action by bitter personal animosities.

Brilliant successes, like those of Galileo, raise up an army of enemies. He was haughty with his own. Sure of his talents, his fortune and his powerful patrons in church and state, he had no managements for any one. 'The wind is fair: now is the time to take in sail,' is a maxim that he would have scorned. Of Aristotle's virtues he practised magnificence, not prudence. His colleagues in the universities were mostly Aristotelians. The heretical and Arab Aristotle had been banished; the Greek Aristotle reigned supreme. Galileo handled his opponents harshly. He was proud; he had a right to be. He was haughty; it led to his fall. When certain chosen astronomers of Italy were asked in 1615 by the Holy Office to report on his system, the report was adverse. Science and pseudo-science were in conflict and the latter won. The Aristotelianism of the universities was bound closely to that of the church. In attacking the orthodox Aristotle, Galileo attacked—or was supposed to have attacked—orthodoxy itself. His enemies were vanquished in philosophy; they dragged in texts of scripture to support the weakness of their science. Galileo met them on this ground also, which was a fatal error. He was no more competent to discuss texts of scripture than they to decide upon points of science.

Father Castelli, an ardent friend of Galileo's, had been appointed to be professor of mathematics at Pisa (1613). At a dinner at the Ducal Palace (December, 1613) the conversation turned on astronomical matters. Did the Medicean stars really exist? asked the Dowager Duchess Christine. The professor of physics in the university reluctantly admitted that they did—that he had seen them. Castelli then praised Galileo's splendid discovery. The professor whispered something to the duchess to insinuate that while the discoveries might be true, the conclusion in favor of the Copernican theory was certainly contrary to scripture. Castelli was called upon to reply and made a brilliant answer. The Grand Duke and most of those present were convinced. Castelli reports all this to Galileo, and Galileo writes in reply (Decem-

ber 21, 1613) a long and eloquent letter on the subject. The original of this letter was never found, although the Inquisition made diligent search for it. Many authentic copies were circulated, however.* The question of the place of the Bible in scientific questions is discussed. Galileo is a good Catholic; the scriptures can not lie or err, he says. But the expositors are fallible. They will fall into error, nay into heresy, if they interpret Holy Writ literally. Both scriptures and external nature owe their origin to the Divine Word.

"It was necessary, however, in Holy Scripture, in order to accommodate itself to the understanding of the majority to say many things which apparently differ from the precise meaning. Nature, on the contrary, is inexorable and unchangeable, and cares not whether her hidden causes and modes of working are intelligible to the human understanding or not, and never deviates from her prescribed laws." It appears to me, therefore, says Galileo, that no effect of nature, which experience places before our eyes, or is the necessary conclusion derived from evidence, should be rendered doubtful by passages of Scripture which contain thousands of words admitting of various interpretations, for every sentence of Scripture is not bound by such rigid laws as is every effect of Nature. . . . Since two truths can obviously never contradict each other, it is the part of wise interpreters of Holy Scripture to take the pains to find out the real meaning of its statements in accordance with the conclusions regarding nature which are quite certain, either from the clear evidence of sense or from necessary demonstration. As therefore the Bible, although dictated by the Holy Spirit, admits . . . in many passages of an interpretation other than the literal one and as, moreover, we can not maintain with certainty that all interpreters are inspired by God, I think it would be the part of wisdom not to allow any one to apply passages of Scripture in such a way as to force them to support, as true, conclusions concerning nature, the contrary of which may afterwards be revealed by the evidences of our senses or by necessary demonstration. Who will set bounds to man's understanding? Who can assure us that everything that can be known in the world is known already? . . . I am inclined to think that the authority of Holy Writ is intended to convince men of those truths which are necessary for their salvation which, being far above man's understanding can not be made credible by any learning, or by any other means than revelation by the Holy Spirit. But that the same God who has endowed us with senses, reason and understanding, does not permit us to use them and desires to acquaint us in any other way with such knowledge as we are in a position to acquire for ourselves by means of those faculties, that it seems to me I am not bound to believe, especially concerning those sciences about which the Holy Scriptures contain only small fragments and varying conclusions; and this is precisely the case with astronomy, of which there is so little that the planets are not even all enumerated

This noble declaration of the independence of man's reason, written in 1613, marks the highest insight yet reached by the human spirit in this regard. It is the greatest product of Galileo's philosophical genius. It was written in haste, he says, yet its form is perfect and

* The letter was subsequently expanded and addressed in its new form to the Grand Duchess Christine (1614).

convincing. It is the weighty expression of convictions felt, pondered over and matured. It precisely expresses the attitude of the generations that followed Darwin. No considerable body of men ever held it before that day. It delighted Castelli and a few of the more enlightened of Galileo's circle. His enemies received it with breathless, uncomprehending rage. They sought for flaws in the argument and, unhappily, they had not far to seek. For, not content with these general principles, Galileo went on to explain certain passages of scripture in a fashion that, at the best, was weak and unconvincing, almost disingenuous. The famous passage in Joshua, 'The sun stood still in the midst of heaven (and hasted not to go down about a whole day)' is expounded by first suppressing the words in parentheses, next by a wire-drawn argument to prove that Joshua's command was given when the sun was near setting (which disagrees with the words purposely omitted) and that 'the midst of heaven' does not mean the place of the sun near noon, but its central place in space among the planets. Hence, says Galileo, this passage actually demonstrates that the sun occupies the center of the world, and refutes Ptolemy. The plain meaning of the verse was distorted by a wilful suppression. It is said in the XIX. Psalm 'The sun's going forth is from the end of the heaven and his circuit unto the ends of it.' Galileo explained this to mean that the sun is the nuptial bed, and the bridegroom coming out of his chamber rejoicing is the light of the sun—his rays—not the sun himself. There is not a shade of reason for this arbitrary interpretation. It is not convincing to us; it was abhorrent to his adversaries. Is it any wonder that they loudly proclaimed their intention to protect the words of the Bible from the profane interpretations of laymen? Into the quicksand of theological interpretation Galileo had no call to enter. He should have declined the controversy thrust upon him by his enemies on the simple ground that he was no more fitted to deal with theology than his adversaries with science. This was, however, not his belief, and he accepted their challenge. By so doing he quite nullified the effect of his noble stand upon general principles. Radical and bold as this stand was, he could have maintained it as Cremonini had maintained his own upon a similar issue. At this critical point in his career two roads were open. He recklessly, even presumptuously, chose the wrong one. All his tribulations are the result of this choice. In two letters of February 16 and March 28, 1615, Galileo, writing to Mgr. Dini, regrets that he has been forced to defend his system against religious scruples. In his letter to the Grand Duchess Christine he had said 'the professors of theology should not assume authority on subjects which they have not studied.' It never so much as crossed his mind that his own interpretations of the

texts of Joshua and the Psalms were like assumptions of authority. In all that follows it must not be forgotten that Galileo had the free choice of leaving the scriptural interpretations alone and of confining himself to science and to philosophical considerations of a general nature. He *chose* to enter the lists, and there is every reason to believe that he felt sure of winning.

Galileo's case recalls that of Roger Bacon, nearly four centuries earlier. The science of both these men of genius was, in the main and essentially, illuminating and correct. It was, for both of them, opposed by ignorant men who feared that which they could not understand. Both of them went out of the province in which alone they had authority, to enter another in which their contemporaries and fellows were at least as well able to judge as they. Both of them overbore and offended their colleagues by harshness. When they were brought to trial those very colleagues were, in turn, accusers, jurors and judges. A like fate befell both.

The history of Jordano Bruno does not fall within the scope of this article and need be considered only so far as it affected the contemporaries of Galileo, and Galileo himself. The following paragraphs from Draper's 'Intellectual Development of Europe' give the views of a writer who is inclined to present Bruno's history in the most favorable light. The foot notes are my own.

Against the opposition it had to encounter, the heliocentric theory made its way slowly at first. Among those who did adopt it were some whose connection served rather to retard its progress, because of the ultraism of their views, or the doubtfulness of their social position. Such was Bruno, who contributed largely to its introduction into England, and who was the author of a work on the Plurality of Worlds, and of the conception that every star is a sun, having opaque planets revolving about it—a conception to which the Copernican system suggestively leads. Bruno was born (1550) seven years after the death of Copernicus. He became a Dominican, but, like so many other thoughful men of the times, was led into heresy on the doctrine of transubstantiation.* Not concealing his opinions he was persecuted, fled, and led a vagabond life in foreign countries,† testifying that wherever he went he found scepticism under the polish of hypocrisy, and that he fought not against the belief of men, but against their pretended belief. For teaching the rotation of the earth he had to flee to Switzerland, and thence to England, where, at Oxford, he gave lectures on Cosmology. Driven from England, France and Germany in succession, he ventured in his extremity to return to Italy, and was arrested in Venice where he was kept in prison in the Piombi for six years without books, or paper, or friends. Meantime the Inquisition demanded him as having written heretical works. He was therefore surrendered to Rome, and,

* Bruno was twice disciplined for 'open and avowed' heresy during the thirteen years of his cloister life (1563–1576). He denied the personality of Christ for one thing.

† Toulouse, Paris (1579 and 1585), Oxford (1583), Wittenburg (1587), Prague (1588), Helmstadt (1589), Frankfort (1590), Marburg (1586), Venice (1592), Rome (1593). These dates correct some errors of the text.

after a further imprisonment of two years, tried, excommunicated, and delivered over to the secular authorities, to be punished 'as mercifully as possible and without the shedding of his blood,' the abominable formula for burning a man alive. He had collected all the observations that had been made respecting the new star in Cassiopeia, 1572; he had taught that space is infinite, and that it is filled with self-luminous and opaque worlds many of them inhabited—this being his capital offense.* He believed that the world is animated by an intelligent soul, the cause of forms but not of matter; that it lives in all things, even such as seem not to live; that everything is ready to become organized; that matter is the mother of forms and then their grave; that matter and the soul of the world together constitute God. His ideas were therefore pantheistic, 'Est Deus in nobis.' In his *Cena delle Cenere* he insists that the Scripture was not intended to teach science, but morals only. The severity with which he was treated was provoked by his asseverations that he was struggling with an orthodoxy that had neither morality nor belief. This was the aim of his work entitled 'The Triumphant Beast.' He was burned at Rome, February 16, 1600.

In 1612 Galileo writes to Kepler that epicycles and eccentrics are not chimerical; 'not only are there many motions in eccentrics and epicycles, but there are no other motions.' This, written three years after Kepler had sent him his Theory of Mars containing the proof of elliptic motion, shows that Galileo had not yet appreciated Kepler's revolutionary discoveries. It is doubtful if he ever did so. He makes no effective use of them in his arguments in favor of the Copernican doctrines.

In the meantime busy enemies were stirring up trouble. The letter to Castelli gave great offense. The Bishop of Fiesole became enraged at Copernicus and was much surprised to learn that he had been dead for eighty years. A Dominican friar, P. Caccini, preached a violent sermon against Galileo (1614) on the text *Viri Galilæi quid statis aspicientes in cælum?* Ye men of Galilee, why stand ye gazing up into Heaven? Castelli was advised by the archbishop of Pisa, 'for his welfare,' 'if he wished to escape ruin,' to abandon the Copernican opinion, because that opinion, besides being an absurdity, was perilous, scandalous, rash, heretical and contrary to scripture.

Another Dominican friar, Lorini, addressed to Cardinal Mellini, president of the Congregation of the Index, a denunciation of 'the Galileists,' who hold the doctrine of Copernicus. The congregation accordingly (February, 1619) opened a secret inquiry. A copy of Galileo's letter to Castelli was examined by the consultator of the Holy

* One of them; his pantheistic ideas were, perhaps, his worst heresies in the eyes of his judges. His doctrine that space is infinite filled the pious Kepler, as well as Bruno's Roman judges, with 'horror.' Bruno's works were full of opinions that were abhorrent to all religious people of his time. He was inclined to pronounce in favor of polygamy, and he advocated a species of socialism. Religion he made essentially synonymous with intellectual culture, neglecting moral discipline and spiritual feeling.

Office, who pronounced that some phrases of it looked ill at first sight, but that they were capable of interpretation in a good sense, and did not deviate from Catholic doctrine. Caccini was summoned to Rome as a witness and gave evidence, most of which was found to be baseless (November, 1615) and was disregarded.

Early in the same year Galileo had sent copies of the letter to Castelli to friends in Rome. It was greatly admired; but his friends, one and all, strenuously advised him to keep to philosophy and to avoid religious discussion. Prince Cesi expressly warns him to avoid all mention of the Copernican theory, for Cardinal Bellarmine—a good, great and powerful prince of the Church—had told him that in his opinion the theory was heretical and contrary to scripture. Cardinals Barberini, Del Monte and Bellarmine assured Galileo's Roman friends that so long as he confined himself to scientific questions and did not enter into theological interpretations of the Bible he had nothing to fear (August, 1615). All these cardinals were very friendly to Galileo personally, and their friendship stood him in good stead. Their attitude was representative of that of the church. So long as religion was not attacked science was to be free. Any assault on doctrine was to be repelled with vigor, and at all costs. Theological interpretation was not to be permitted to laymen. That was a business reserved by the church.

A Carmelite monk, Foscarini, printed in 1615 a letter on 'the opinion of the Pythagoreans and of Copernicus of the mobility of the earth and the stability of the sun,' which was widely read and quickly came to a second edition. The Inquisition was at this time considering Foscarini's book also. Galileo felt that his presence at Rome would be advantageous, and in December, 1615, he set out provided with letters of introduction from the Grand Duke to dignitaries, including the Tuscan ambassador, Guicciardini. He was received with honor as a celebrity. With no great effort he freed himself from all personal difficulties and was able to report (February 6, 1616) that the monk Caccini had made him a formal visit to ask his pardon. On the same day he writes to the Tuscan Secretary of State, Piechena: "My business, so far as it relates to myself, is completed. All the exalted personages who have been conducting it have told me so plainly and in a most obliging manner. . . . So far as this point is concerned, therefore, I might return home without delay."

He goes on to say that it is proposed to pass judgment upon the Copernician doctrine, and that it is his conviction that he may be of use in the investigation of the matter, on account of his scientific knowledge. Accordingly he proposes to stay. He had been personally vindicated. It was his ardent desire to convert the Romans to the heliocentric theory. This he attempted by giving private lectures in

many of the great houses of Rome. His lectures began by stating all the arguments in favor of Ptolemy's system and then went on to demolish them one by one, leaving nothing standing. The lectures were admired by many great folk, and Galileo gained a great personal success for the time. His very success made his well-wishers uneasy and unquiet.

Before Galileo's visit, Fra Paolo Sarpi, professor of philosophy in Venice, distinguished as a champion of free thought and as a friend of Galileo had written: "I hear that Galileo is going to Rome, where he is invited by several Cardinals to explain his new discoveries in the heavens. I fear much that, in such a case, he may develop the reasons that lead him to prefer the doctrine of Copernicus, which will be far from pleasing to the Jesuits and other monks. They have changed what was only a question of physics and astronomy into a theological question, and I foresee, with great vexation, that Galileo, in order to live in peace, and not labeled as heretic and excommunicate, will be constrained to abjure his real sentiments on this matter. A day will come, of that I am almost sure, when enlightened men will deplore the misfortune of Galileo and the injustice done to so great a man. But, pending that day, he must suffer, and he must not complain otherwise than secretly."

The Tuscan ambassador at Rome was anxious to be rid of Galileo, and in many letters reports that it were well he returned home. He hints that Galileo's course may even bring dangers to Tuscany; he can not 'approve that we should expose ourselves to such annoyances and dangers without very good reason.' He insinuates that Cardinal Carlo de Medici may be compromised (March 4, 1616). "Galileo seems disposed to emulate the monks in obstinacy, and to contend with personages who can not be attacked without ruining yourself; we shall soon hear at Florence that he has madly tumbled into some abyss or other." "The moment is badly chosen to promulgate a philosophical idea." The Grand Duke, from friendliness to Galileo and in fear of untoward complications, gave instructions for his recall, which were conveyed in a dispatch from the ducal secretary: "You have had enough of monkish persecutions. . . . His Highness fears that your longer tarrying at Rome might involve you in difficulties, and would therefore be glad, as you have so far come honorably out of the affair, if you would not tease the sleeping dog any more, and would return here as soon as possible. For there are rumors flying about which we do not like, and the monks are all powerful." Galileo set out for Florence on the fourth of April, 1616.

Let us stop for a moment to inquire what the course of affairs would have been if Galileo, whose personal affairs were honorably concluded on February 6, had thereupon returned to Florence. He had

renewed old friendships; he had formed new ones; he was esteemed and regarded by the Pope and the most influential of the Cardinals. His enemies in Florence were utterly silenced. His accuser, Caccini, had made the humblest apologies. The Grand Duke and most of the court were his admiring friends. He had every freedom for research if only he would leave the interpretation of scripture to theological experts. 'Write freely, but keep outside the sacristy' his friends advised. Why did he remain in Rome? To convert the Congregation of the Index to Copernicanism? This would have been a triumph for science, and a personal triumph as well. The Roman Curia had absolutely no interest in science as such. They were determined that religion should not suffer. Galileo's brilliant lectures were not conceived in the spirit that convinces. He silenced opposition by sarcasm. A second crisis in Galileo's affairs dates from this period (February, March, 1615).

Before this date momentous action had been taken by the Inquisition. On February 19 the Qualificators of the Holy Office had been summoned to give their opinion on two propositions based on Galileo's treatise on the Solar Spots:

I. That the sun is the center of the world and immovable from its place.

II. That the earth is not the center of the world, nor immovable, but moves, and also with a diurnal motion.

The Qualificators were to give their opinion as theological and philosophical experts, and gave it four days afterwards. The astronomer Riccioli declares that the opinions of astronomical experts were also obtained and that the judgment of the Holy Office was based upon them (Delambre: *Histoire de l'Astronomie Moderne,* i., 680). There is no reason to doubt the assertion. It is exceedingly important as showing that the Inquisition took the best expert advice known to them before action. This significant fact is not mentioned in any of the Warfare-of-Science books, nor even by so careful an historian as Gebler.*

The scientific value of the expert astronomical opinion was, of course, exactly *nil.* It was given, probably, by Aristotelians, personally inimical to Galileo, and fully committed to the Ptolemaic system. It was, equally of course, adverse to Galileo. They may well have

* Gebler records, however the action of Cardinal Gaetano who, in 1616, applied to Thomas Campanella, a learned Dominican and a friend of Galileo's, for an opinion upon the relation of the Copernican theory of Holy Scripture. Campanella's 'Apology' for Galileo was all in his favor and reconciled, in form at least, Copernican science with the Bible. It was overweighed by other reports. It is worth recording that Campanella was not permitted to publish this 'Apology' in Italy and was obliged to disavow an edition which appeared at Frankfort.

quoted the dictum of Tycho Brahe that the system of Copernicus was 'absurd and contrary to Holy Writ' since the judgment recites these very words. On March 5, 1616, the 'De Revolutionibus' of Copernicus and another work by Diego di Zuniga were suspended by the Congregation of the Index 'until they be corrected,' and Foscarini's book was 'altogether prohibited and condemned' as well as 'all other works' in which the Copernican opinion is taught. On February 25 the Pope directed 'Cardinal Bellarmine to summon before him the said Galileo and to admonish him to abandon the said opinion; and, in case of his refusal to obey, that the Commissary is to intimate to him, before a notary and witnesses, a command to abstain altogether from teaching or defending this opinion and doctrine, and even from discussing it; and if he do not acquiesce therein, that he is to be imprisoned.' This document is followed in the Vatican MS. by another: "Friday, the 26th (February, 1616). At the Palace, the usual residence of the Lord Cardinal Bellarmine, the said Galileo, having been summoned and brought before the said Lord Cardinal, was in the presence of the Most Rev'd Michael Angelo Segnezzio, . . . Commissary-General of the Holy Office, by the said Cardinal warned of the error of the aforesaid opinion and admonished to abandon it; and immediately thereafter, before me and before witnesses, the Lord Cardinal Bellarmine being still present, the said Galileo was by the said Commissary commanded and enjoined . . . to relinquish altogether the said opinion . . . ; nor henceforth to hold, teach or defend it in any way whatsoever, verbally or in writing; otherwise proceedings would be taken against him in the Holy Office; which injunction the said Galileo acquiesced in and promised to obey. Done at Rome in the place above said, in presence of (two persons named) witnesses." This annotation was long supposed to have been fabricated in 1632 to meet new conditions then arising. It is, however, entirely genuine. (Gebler's 'Galileo,' Appendix III.)

The exact wording is to be noted. Upon this admonition the subsequent fate of Galileo hangs.

(To be continued.)

HOW IMMIGRANTS ARE INSPECTED.

BY DR. ALLAN McLAUGHLIN,
U. S. PUBLIC HEALTH AND MARINE HOSPITAL SERVICE.

Inspection of our immigrants may be said to begin in Europe. The immigrant usually buys his steamship ticket in his native town from an agent or subagent of the steamship company. The agents of the best steamship lines are held responsible by the company, for the passengers they book for America, and if they ship one of the excluded classes they are likely to lose their agency. This makes the agent examine the applicants for tickets, and probably quite a large number of defectives are refused passage by agents of the first-class lines. These defectives then usually try some less particular and smaller lines and take chances of escaping inspection at the Canadian or Mexican borders.

The next scrutiny to which the immigrant is subjected is that of the steamship authorities at the port of embarkation. This was formerly a perfunctory examination, and is so still, as far as some lines are concerned, but first-class lines, notably the English and German, examine the immigrants carefully and with due regard for our laws. The strict enforcement of our laws, and especially the imposition of one hundred dollars fine for bringing to our ports any case of a contagious character, have occasioned some improvement in the inspection made by ships' doctors at European ports. At the port of embarkation the immigrants' names are recorded upon lists or manifests, each list containing about thirty names. After each name the steamship officials are required by law to record answers to a certain number of queries relating to the immigrant.

Section 12 of the act of 1903 provides that the manifests shall state, in answer to the questions at the top of the manifest sheet:

> The full name, age and sex; whether married or single; the calling or occupation; whether able to read or write; the nationality; the race; the last residence; the seaport of landing in the United States; the final destination, if any, beyond the port of landing; whether having a ticket through to such final destination; whether the alien has paid his own passage, or whether it has been paid by any other person or by any corporation, society, municipality, or government, and if so, by whom; whether in possession of thirty dollars, and if less, how much; whether going to join a relative or friend and if so, what relative or friend, and his name and complete address; whether ever before in the United States, and if so, when and where; whether ever in prison or almshouse or an institution or hospital for the care and treatment of the

insane or supported by charity; whether a polygamist; whether an anarchist; whether coming by reason of any offer, solicitation, promise or agreement, expressed or implied, to perform labor in the United States, and what is the alien's condition of health, mental and physical, and whether deformed or crippled, and if so, for how long and from what cause.

The master or first officer and the ship's surgeon are required by the same law to make oath before an immigration officer at the port of arrival that the lists manifests are to the best of their knowledge and belief true, and that none of the aliens belongs to any of the excluded classes. Each alien is furnished with a card, with his name, the number of the list on which his name appears and his number on that list. The cards of minor children are given to the head of the family. These cards are valuable and necessary for identification, and facilitate inspection at the port of arrival.

The condition of the steerage quarters of a modern steamship depends largely upon the age of the ship and the degree of overcrowding. The steerage of a first-class ship of recent construction will afford accommodations equal to those accorded second cabin passengers on less progressive lines. First-class lines are careful also to prevent overcrowding. On some of the smaller and older ships the accommodations are limited, and overcrowding is permitted. But it is safe to say that the worst steerage accommodations to be found on any ship entering New York harbor to-day are infinitely better than the best afforded by the sailing vessels or old 'side wheelers' of the past.

On entering New York harbor the ocean liners are boarded by the state quarantine authorities, and the immigrants inspected for quarantinable disease, such as cholera, small-pox, typhus fever, yellow fever or plague. Then the immigrant inspectors and a medical officer of the Public Health and Marine Hospital Service board the vessel and examine the cabin passengers, paying particular attention to the second cabin. This cabin inspection is very necessary, and, before its institution, the second class cabin was the route most often employed by persons who found it necessary to evade the law. After the completion of the cabin inspection the ship's surgeon reports any cases of sickness among the aliens in the ship's hospital. The medical inspector examines these cases and later arranges for their transfer, if deemed advisable, from the ship to the immigrant hospital. The immigrants are then taken from the ship upon barges to the immigrant station, Ellis Island.

The medical examination at Ellis Island is conducted according to a system which is the result of many years of development. The doctors work in pairs, and divide the inspection between them. The immigrants, coming in single file, are examined for certain defects by the first doctor, who detains each one long enough to keep a space of

ten to fifteen feet between the immigrants. The second doctor, placed about thirty feet from the first, disregards that part of the examination entrusted to his colleague and confines his examination to such defects as are not looked for by the first doctor. The file of immigrants makes a right-angle turn just as it reaches the second doctor and this enables the examiner to observe the side and back of the passenger in the shortest time possible.

The examiners follow a routine in this examination, and the scrutiny begins at the approaching passenger's feet, before he comes within fifteen feet of the examiner. The examiner's scrutiny beginning at the feet travels upward, and the eyes are the last to be inspected. In this way, lameness, deformity, defective eyesight (through efforts to adjust his vision, after making the turn, to a new course) are detected. The gait and general appearance suggest health or disease to the practised eye, and aliens who do not appear normal are turned aside, with those who are palpably defective, and more thoroughly examined later.

The medical examiners must ever be on the alert for deception. The nonchalant individual with an overcoat on his arm is probably concealing an artificial arm; the child strapped to its mother's back, and who appears old enough to walk alone, may be unable to walk because of infantile paralysis; a case of favus may be so skilfully prepared for inspection that close scrutiny is required to detect the evidences of recent cleansing, and a bad case of trachoma may show no external evidence and be detected only upon everting the eyelid.

After the last alien in line has passed the doctor, the suspected ones turned aside are thoroughly examined, idiots and those suffering with a loathsome or dangerous contagious disease are certified and sent to the board of special inquiry. Cases not deemed fit to travel are sent to the hospital, and cases with some disability likely to make them a public charge are certified accordingly and also sent to the board of special inquiry. Minor defects, such as anemia, loss of an eye, loss of a finger, poor physique, low stature, etc., are recorded on the alien's card and he is allowed to go to the registry clerk and immigrant inspector in charge of the manifest, who takes the defect into consideration as contributory evidence, and may or may not send him to the board.

After passing the doctors, the immigrants are grouped, according to the number of their manifest sheet, into lines of thirty or less. At the head of each line is a registry clerk, or interpreter, and an immigration inspector. The clerk, or interpreter, interrogates each alien, and finds his name, and verifies the answers on the manifest sheet before him, and if, in the opinion of the immigrant inspector, the immigrant is not clearly and beyond doubt entitled to land, he is held for the consideration of the board of special inquiry. A board of spe-

cial inquiry according to the law of 1903 'consists of three members selected from such of the immigrant officials in the service as the commissioner general of immigration, with the approval of the secretary of commerce and labor, shall designate as qualified to serve on such boards.' "The decision of any two members of a board shall prevail and be final, but either the alien or any dissenting member of said board may appeal through the commissioner of immigration at the port of arrival, and the commissioner general of immigration to the secretary of commerce and labor, whose decision shall then be final, and the taking of such appeal shall operate to stay any action in regard to the final disposal of the alien, whose case is so appealed, until receipt by the commissioner of immigration at the port of arrival, of such decision." To this 'board of special inquiry' are sent the aliens certified by the medical officers as suffering from loathsome or dangerous contagious disease, idiocy, epilepsy and insanity.

In cases so certified the law is mandatory, and the medical certificate is equivalent to exclusion, the board simply applying the legal process necessary for deportation. Aliens certified by the medical officers as suffering from disability, likely to make them public charges, are also held for examination before the board of special inquiry. The board in these cases takes into consideration the medical certificate and such evidence as may be adduced by the alien or his friends which, in the opinion of the board, would offset the physical disability. In these cases the board has full discretionary powers, and in a great majority of instances the alien is admitted. Those certified as defective by the doctors group themselves naturally into four classes, and the following table indicates the disposition of such cases by the boards of special inquiry at New York during a fairly representative month:

DISPOSITION OF IMMIGRANTS CERTIFIED AT ELLIS ISLAND, N. Y., MONTH OF OCTOBER, 1903.

	Class I. (Dangerous Contagious.)	Class II. (Insanity and Idiocy.)	Class III. (Loathsome.)	Class IV. (Likely to become a Public Charge.)
Cases pending beginning of month..	10	0	0	30
Cases certified during month.....	83	1	1	393
Total to be accounted for.....	93	1	1	423
Cases deported.....	61	1	1	30
Cases landed	4	0	0	349
Cases pending close of month.....	28	0	0	44

Immigrants not detained for the board of special inquiry have their money changed into United States currency, and buy their railroad tickets, under the supervision of government officers. If they are destined to points beyond New York City, government supervision is maintained until they are taken to one of the great railroad terminals

and placed upon the waiting train. These precautions are taken to protect the immigrants from the boarding house 'runners' and other sharpers who lie in wait for them at the Battery. Aliens detained as not clearly entitled to land are brought before the board, and, if the evidence is complete, either deported or discharged. When the evidence is incomplete, the immigrant is detained pending the verification of his story, or the arrival of his relatives or friends. All cases are disposed of as rapidly as possible, and immigrants are detained the minimum amount of time required for procuring and carefully considering the evidence in the case. Those ordered deported are returned to the ship as soon as possible after the decision is rendered, providing no appeal is made.

Missionaries and representatives of various religious denominations and societies have offices upon Ellis Island and render valuable assistance to the immigrant. They provide temporary shelter and protection for discharged aliens, and direct them to legitimate employers of labor. In this way they relieve the government of caring for many temporarily detained aliens, especially young women traveling alone. They write letters and send telegrams to the friends of the detained immigrants, and assist them in many other ways.

The fine adjustment of details and perfection of system which enable the federal officers at Ellis Island to examine, under our laws, thousands of aliens each day must be seen to be fully appreciated. Nor is this careful and strict execution of our laws limited to Ellis Island. The writer has roughly described the inspection at New York, because it is our largest port of entry, but the same attention to detail and strict enforcement of laws and regulations can be said to exist at all our ports, and an investigation, by any one interested, will reveal the fact that not only are the laws for our protection strictly enforced, but their enforcement is marked by humane and kindly treatment of the alien.

ON THE RELATIONS OF THE LAND AND FRESH-WATER MOLLUSK-FAUNA OF ALASKA AND EASTERN SIBERIA.

By WILLIAM HEALEY DALL,
SMITHSONIAN INSTITUTION.

HAVING recently completed a census of the land and fresh-water mollusk-fauna of Eastern Siberia and Alaska, together with a discussion of the relations of its elements, it seemed that a summary of the results in their bearing on geographical distribution of animal life in the boreal regions might have some interest for the readers of the POPULAR SCIENCE MONTHLY.

In the region north of latitude 49° the molluscan population of North America is rather scanty. For distributional purposes it must be divided into two series, one containing the aquatic forms and the other the land shells. The distribution of water animals is carried on by different means from those influential in the dispersal of terrestrial forms, and any discussion which combined the two without distinction would be liable to contain errors of fact and deduction.

The vast territories under consideration have a number of drainage systems which in the tabulation of species should be distinguished. The chief of these are:

1. *The Labradorian.*—This comprises the area of drainage into Ungava Bay and the Atlantic north of the straits of Belleisle and the 'Height of Land,' including the Labrador coast and the northeastern part of the Ungava District of the Dominion of Canada.

2. *The Canadian.*—This system comprises the drainage of the St. Lawrence and the Great Lakes south and east from the 'Height of Land,' including the island of Anticosti.

3. *The Hudsonian.*—This, the largest system of all, includes the entire area draining into Hudson Bay, including Keewatin, the southeastern corner of the Mackenzie District, eastern Athabaska, the whole of Saskatchewan, the southeastern two-thirds of Alberta, Assiniboia and Manitoba, the drainage area of the Red river of the north in the Dakotas, and northeastern Minnesota, all of Ontario, Quebec, and Ungava north and west of the 'Height of Land.'

4. *The Mackenzian.*—This vast system includes the basin drained by the Mackenzie river and its tributaries, covering northwestern Alberta, northeastern British Columbia, and the northwestern two thirds of Athabaska and the Mackenzie District.

5. *The Columbian.*—This includes the coast drainage of British Columbia and Southeastern Alaska, the basins of the Fraser and Columbia rivers, the coastal part of the State of Washington, and the northern part of Idaho and Montana, west of the Selkirk Range and its more southern equivalents in the Rocky Mountain region. The northwestern extension of this system in Alaska, between the Coast Ranges and the sea, including the Alaska Peninsula and the Aleutian Islands, being really an extension northwestward of the Columbian system, might perhaps for convenience be called the Alaskan subregion.

6. *The Yukonian.*—This system includes the entire drainage of the Yukon river, the tundra north of it and the basin of the Kuskokwim; or all of Alaska north, northwest and west of the Alaskan range, as well as that of the basin east of the Coast ranges drained by the Yukon and its tributaries.

We know through the researches of General G. K. Warren, U. S. Engineers, that a considerable portion of the original Mississippi drainage near its former headwaters has, through changes of level of the earth near the 49th parallel, been captured by the Hudsonian drainage system, and it is not unreasonable to suppose, that a certain proportion of the Mississippi fresh-water fauna was captured with the streams in which it lived. Some such supposition seems necessary to explain the far northern extension of certain Mississippi Valley species in the Hudsonian region, while other drainages equally suited to their inhabitation are destitute of them.

Another factor in the distribution of land as well as fresh water shells is the former existence of a continental ice-cap by which the entire mollusk fauna of the region occupied by ice would be exterminated; though, in the interglacial periods, the external fauna might advance as the ice retreated, only to be driven out again on the return of the ice. Such fluctuations have been well shown, by the researches of Dr. Coleman, of Toronto, to have occurred in the valley of the Don, near that city.

The vast territories included in these drainage systems are, it is true, only partially and imperfectly explored for mollusks. Yet certain portions of them are tolerably well known and the uniformity imposed on the fauna by its high northern position and unvaried conditions, leads to the belief that, while much is yet to be known in tracing out the details of distribution, little is to be expected in the way of absolutely new species, even from this immense territory yet to be explored.

It would be rash to conclude that nothing new remains to be found, but our expectations should certainly be moderate.

There are a few characteristic fresh-water forms in Greenland,

Labrador and the Hudsonian region, but in the main, the molluscan population of the waters of the region described may be regarded as a northward extension of the Northeast American and Mississippi Valley faunas, diminished by the elimination of all the less hardy species and reinforced by the addition of a small number of circumboreal forms. The total fresh-water fauna comprises 138 species, of which 90 occur in the Hudsonian system, but only 23 extend to the Mackenzie basin, though the Yukon system may boast of 29. Some eccentricities of distribution will be referred to later.

The distribution of land shells is not affected to any serious extent by rivers or streams, but is doubtless, in the main, the result of the slow movement of individuals.

The pulmonate fauna of Alaska is composed of four elements; contributions from the faunas of Asia, of the Pacific Coast region of America, of the Hudsonian or Canadian region, and of the special circumboreal or common subarctic fauna of the whole northern hemisphere.

Differences of latitude mean for the snail not so much differences of temperature corresponding to the latitude. as differences of the annual active period, which diminishes as one proceeds northward. Snails at Point Barrow (and there are such) must remain in a state of hibernation at least nine months in the year, and I suspect that this more probably brings a limiting strain on the vitality of the organism, than would the mere occurrence at times of a specially low temperature.

The Alaskan land shells number 86 species, of which 16 are circumboreal, and the same number (though not all the same identical species) are common to the northeastern part of Siberia. Fifty species are held in common with the Pacific fauna, while 34 are part of the Yukon fauna which is so intimately allied to that of the Hudsonian region. These latter two groups are limited by topographic features. Thus the fauna of the Hudsonian region, in constantly diminishing number of species, is extended to the northwest through the Yukonian region north of the Coast range and the Alaskan range, to Bering Sea on the west and the Arctic coast on the north.

In like manner the fauna of the Columbian region of the Pacific Coast, is extended along the Alaskan coast and islands south and west of the two ranges mentioned, and between them and the sea. Some of the most characteristic and larger species are cut off in their northwestern extension by the area near Mount St. Elias, where along one hundred miles of coast immense glacier fields extend to the very border of the sea. The last representatives of this fauna disappear among the eastern Aleutian islands. In British Columbia a few species belong to the Valley region between the Rocky and the Cascade Mountains, and do not reach the sea coast, but these are too few to

modify essentially the general rule, and like the valleys themselves, these species disappear a short distance north of the 49th parallel. I was able to show, some thirty-five years ago, that in a broad way the distribution of the birds and plants presents analogous features.

Fauna of Northeastern Asia.

The land shell fauna of the northeast extremity of Asia has little individuality. but represents a mingling of the depauperated extremes of the faunas of northeast China and of Europe with that series of species which is sometimes called the circumpolar or circumboreal fauna.

Much of the apparent poverty of the fauna may be due to insufficient collecting, but even when the most generous allowance for this factor is made, it still remains certain that the molluscan population is far less in variety than might reasonably be expected.

The palaearctic fauna of Europe appears to extend clear across Northern Asia, losing a large proportion of its species on the way, until (if the circumboreal species be excluded) only about thirty species reach the headwaters of the Lena and the barrier of the Stanovoi range. A very remarkable local fauna exists in the great 'Relictensee' of Siberia, Lake Baikal, but it does not appear to have tinctured the East Siberian fresh-water fauna outside of that lake, to any appreciable extent. It is possible that the comparatively recent emergence of a large part of Eastern Siberia from the sea, and the presence of the vast desert region to the south and west, may enter into the explanation of this sparse shell-fauna, as well as of some of the peculiarities of the Baikal faunula.

Southeast of the Stanovoi range we find between the mountains and the sea, the valley of the Amur and several smaller valleys, such as the drainage-basins of the Ud and the Tugar. To the southwest the sources of the Amur emerge from the deserts of Gobi and Dauria, and along the line of these water courses has crept a certain number of molluscan forms intimately related to or identical with those of Mongolia, China and the Orient. This forms the second element of the fauna of Northeast Siberia.

The number of purely endemic species is remarkably small and a portion of those claimed to be of this character are probably mere local mutations of widespread palearctic forms already known. Yet it would seem as if a more thorough exploration must add largely to the species now known, and it is almost incredible that the luxuriant fertile valleys of Kamchatka and the innumerable streams and lakes of that country should not be well populated with mollusks.

There are a few species which seem to be common to the shores of Bering Sea, both Asiatic and American, such as *Succinea chrysis, Pyramidula pauper, Punctum conspectum,* and *Anodonta beringiana.*

There is one local species, *Eulota weyrichi,* known only from Sakhalin Island; and another, *Helicigona subpersonata* from the valley of the Ud. Three forms of *Vivipara* (of which two are probably variants of Chinese forms) are the only exclusively local species of the vast Amur Valley, or drainage, not known from other regions. Nine specially Kamchatkan species have been described, but about half of them are doubtfully distinct.

The total number of land and fresh-water mollusks known from the Amurland, Sakhalin, Kamchatka, the Chukchi peninsula, and the Asiatic coast north of the Amur and east of the Stanovoi range, is only eighty-one. Of these thirteen are circumboreal species, and twelve are supposed to be locally peculiar. Of the remainder

Europe and West Siberia contribute.....	55 per cent.
Northeast China contributes.....	22 "
In common with America there is.....	13 "
Erratic species	10 "

Of these erratic species a few may be especially mentioned. *Margaritana margaritifera,* as is well known, is absent from the whole of the great northern central region of North America, though it appears in the Lower Saskatchewan, the sources of the Missouri and in eastern Canada, while on the Pacific it ascends at least to latitude 56° N. In eastern Asia it is known from Kamchatka, Sakhalin Island, the upper portion of the Amur basin, and southern Mongolia—but I find no authoritative record of it thence westward to Northern and Middle Europe. Schrenck did not find it on the lower Amur.

Phisa fontinalis is reported from the upper Amur and (in a duck's crop) the desert of Dauria, but is not known from Siberia proper, though common in Europe. There is an entire absence of typical *Physa* throughout East Siberia, so far as reported; and only one species of *Ancylus* or *Unio* is known from east of the Yenisei river of Siberia.

Aplexa hypnorum is known from Northern Europe, Western Siberia and the Chukchi peninsula, but has not been reported from Eastern Siberia, or the Amur, though abundant in Alaska, and reaching on the Taimyr peninsula to 73° 30′ North Latitude.

Zoögenites harpa is known from Northeastern Scandinavia in Europe; from Northeastern America, the Hudson Bay territory and Southeastern Alaska, in America; but in Siberia it is recorded only from the easternmost margin; the Chukchi peninsula, Bering Island, Kamchatka and the lower Amur. These singularities of distribution must await much more extended knowledge before they can be adequately discussed, but it is believed that to some extent they are due to the transgression of the sea, or of glacial ice, over part of the area in which a species might naturally be expected to occur, thus delaying the occupation of the entire region by the species concerned.

EXAMINATIONS, GRADES AND CREDITS.

BY PROFESSOR J. McKEEN CATTELL,
COLUMBIA UNIVERSITY.

THE determination of individual differences, the improvement of useful traits and the assignment of men to the work for which they are fit are among the most important problems in the whole range of pure and applied science. The extraordinary growth of the material sciences with their applications during the nineteenth century requires as its complement a corresponding development of psychology. It would under existing conditions be intolerable to erect a building without regard to the quality and strength of materials, to use at random a wooden beam or a steel girder; yet we often do much this thing in selecting men for their work and adjusting them to it.

In examinations and grades we attempt to determine individual differences and to select individuals for special purposes. It seems strange that no scientific study of any consequence has been made to determine the validity of our methods, to standardize and improve them. It is quite possible that the assigning of grades to school children and college students as a kind of reward or punishment is useless or worse; its value could and should be determined. But when students are excluded from college because they do not secure a certain grade in a written examination, or when candidates for positions in the government service are selected as the result of a written examination, we assume a serious responsibility. The least we can do is to make a scientific study of our methods and results.

Grades assigned to college students have some meaning, though just what this is remains to be determined. Dr. Wissler* has shown that there is a decided correlation in the standing in different subjects. A man who receives a high grade in Latin is likely to receive a high grade in Greek, and almost as likely to receive a high grade in mathematics or gymnastics. This seems to indicate that the grades are assigned for moral traits, or for the general impression made by the man, as much as for ability and performance in a given subject. Professor Thorndike and his students† have found a similar relationship in school grades and in the New York State Regents' examinations. Professor Dexter‡ has shown that a man who is given a high

* 'The Correlation of Mental and Physical Tests,' Monograph Supplement to *The Psychological Review*, No. 16.

† Summarized in 'Educational Psychology,' Lemcke and Büchner, 1903.

‡ 'High Grade Men in College and Out,' POP. SCI. MON., March, 1903.

standing in college is more likely than others to find his name in 'Who's Who in America.' Phi Beta Kappa men (on the average the upper seventh of the class) are twice as likely to be there as others, and the first man in his class is five times as likely.*

It is evident that subjects differ greatly in examinability. The results of an examination in mathematics, for example, can be graded with considerable accuracy; they give fairly definite information as to the man's mathematical aptitudes, and mathematical ability is largely innate, so that here the boy is father to the man. The mathematical tripos at Cambridge is a real test. Of the fifty senior wranglers in the first half of the last century a very large number have attained eminence. For example, two of them, Sir George Gabriel Stokes and Dr. N. M. Ferrers, who died within a month preceding the writing of this paragraph, maintained both in mathematical performance and general efficiency the position of, say, first in a hundred given them as the result of a student examination. Two facts should, however, be borne in mind. The senior wrangler is given great opportunity by being made a fellow, and the examination is on three years of solid work. The results of examinations in scrappy courses lasting half a year are not nearly so valid.

Subjects such as literature and psychology do not lend themselves to written examinations so well as mathematics. I have had the same papers in psychology graded by different examiners and have found great variations in the results. There is some validity in the order of excellence, but scarcely any in the absolute grades, the variation of the grades for the same paper by different examiners being as large as the variation of different papers by the same examiners. I have not, however, confirmed this result by sufficient data. One of our courses in psychology is given by different instructors, each of whom sets and grades papers for the same student. The grades assigned are A, B, C, D and F—excellent, good, fair, poor and failure. Four instructors gave twenty-one men a total of 15 A's, 38 B's, 27 C's, 4 D's and 1 F. When, however, we average the grades of the four instructors, we get 3B + 17C + and 1D +. All the grades are alike within the unit used, except four, and the probable errors of three of the four show that they are as likely as not to fall within this grade, while the probable error of the remaining grade gives it but moderate validity.

It seems scarcely possible to determine what students are fitted for a college course by means of a written examination; and I fear

* It must, however, be remembered that the kind of people who are put in a book such as 'Who's Who' are largely those who talk about things rather than those who do things—the class that receives part payment for its services in notoriety.

that the systematization of entrance examinations under the auspices of a board will be harmful to secondary education.* The German method, which has made some progress here, of leaving the decision to the school seems much better. If we can not accept the recommendation of the school, I should prefer to see the candidate passed upon by two psychological experts. If their independent judgment agreed, I should have more confidence in this than in the results of any written examination. In general, I should admit to college any students who were not pronounced unfit by expert opinion, dropping of course those who subsequently proved themselves unfit. Requiring all students to pass an examination in Latin composition and the like is as out of place in a modern university as an ichthyosaurus on Broadway†.

Our college entrance requirements and examinations are a serious injury to secondary education, and they select very imperfectly the men who should have a college education. Of 262 students who entered Columbia College in 1900, only 50 completed the regular four-year course in the college. Civil service examinations often exclude the fit from the public service. In Great Britain the method is carried to an extreme, and the results depend as much on the coach as on the candidate. Almost anything is better than appointments for party service; but past performance, character, habits, heredity and physical health are much more important than the temporary information that can be but imperfectly tested by a written examination. I should not be willing to select a fellow or an assistant in psychology by such a method, and to select a professor would be nearly as absurd as to choose a wife as the result of a written examination on her duties. To devise and apply the best methods of determining fitness is the business of the psychological expert, who will probably represent at the close of this century as important a profession as medicine, law or the church.

I am at present working at the problem of assigning grades for moral, mental and physical traits‡, but shall here confine myself

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* Since this was written Professor Thorndike has compiled statistics, not as yet published, which indicate that students who pass these examinations with the lowest grades are as likely to do well in college as those having much higher grades. Those rejected would probably do equally well.

† In the discussion now in progress at Cambridge concerning the requirement of Greek at entrance, Professor Jebb ridiculed New Zealand as a Greekless land, because one of its citizens is alleged to have called Andromache 'Andromach.' Professor Jebb in his speech called New Zealand a part of Australia; yet he does not regard himself as illiterate.

‡ Cf. articles in *Science* (N. S. 17: 561–570, 1903) and *Am. Jour of Psychol.* (14: 310–328, 1903).

to a discussion of college grades. The literature is very scanty. I can only refer to two papers*, both of which are slight.

Grades are usually assigned on a scale of 100, some institutions, as Harvard and Columbia, reporting only the five groups into which the men are divided. The starting point in all grades is the fact that the written papers or the results of the term's work can be arranged more or less accurately in the order of merit.† The assignment of quantitative grades to a qualitative series or its division into groups is usually done in an arbitrary manner, and, so far as I am aware, no attempt has hitherto been made to assign probable errors. It is obvious that our grades should be standardized. Our colleges are in the position of a grocer who should let each of his clerks give to customers without weighing and without knowledge of market prices what he believed to be a dollar's worth of tea.

The simplest method of assigning grades is to arrange a hundred papers as nearly as may be in the order of merit and to give the poorest paper the grade 1, the next poorest the grade 2, and so on, until the best paper receives the grade 100. The 100 cases would not be exactly representative of the entire group with which we are concerned; but if we had 100,000 cases, the error from this source in giving the poorest 1,000 the grade of 1, etc. would be entirely negligible. It is possible to calculate how likely it is that in a random group of 100 cases we should find two, three or more men to whom the lowest or any other grade should be assigned. Each instructor forms a rough estimate of the group of students with which he is concerned, and can with a probable error that might be determined assign its place in the series to each case.

If men are arranged in this way in the order of merit and each is assigned his position in the series from 1 to 100, the differences between them will not be equal. If a hundred men are placed in a row according to height, the line passing along the tops of their heads will not be a straight line. The men in the middle of the row will differ but little from one another, and the differences will become continually greater towards the ends. Fig. 1, (page 366) shows the approximate distribution in stature of 1,052 English women, measured for Professor Karl Pearson. Their average height was about 5 feet 2½ inches; 18.3 per cent. of the whole number were between 62 and 63 inches, and one half of them were within about 1½ inches of the average, the probable error. The ordinates or vertical lines are pro-

* 'American Titles and Distinctions,' W. Le Conte Stevens, THE POPULAR SCIENCE MONTHLY, 63: 312–320, 1903. 'The Education of Examiners,' E. B. Sargent, *Nature*, 70: 63–65, 1904.

† Many instructors doubtless let the grade represent the percentage of questions correctly answered. This is a possible but fallacious method in a subject such as mathematics; in a subject such as psychology it is impossible.

portional to the number of women falling within the limits of an inch. Thus 16.3 per cent. were between 63 and 64 inches; 11.5 per cent. between 64 and 65 inches, etc., only two falling between 70 and 71 inches. The women near the average tend to differ in height by about 1/200th of an inch, while the tallest or shortest of the thousand tend to differ by half an inch or more. This curve, showing the

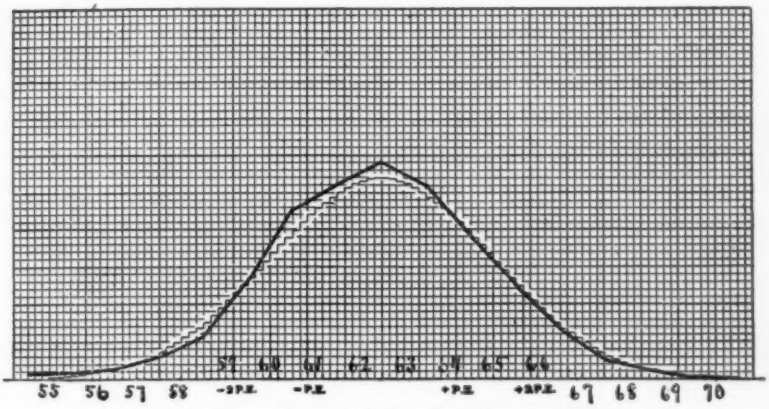


FIG. 1. DISTRIBUTION OF STATURE OF WOMEN IN INCHES.

distribution in height, corresponds closely with the fainter and more regular curve on the figure which represents the distribution of events due to a large number of small causes equally likely to affect them in one of two ways, the curve of error of the exponential equation whose properties have been discussed by Gauss, Laplace and other mathematicians.

If the performances of students in examinations are assumed to vary in the same way as their height, then we can if we like place them in classes which represent equal differences. Thus by the Harvard-Columbia method of grouping into five classes, if we put half the men into the middle class, C, and let B and D represent an equal range, we should give about 23 per cent. of both B's and D's and about 2 per cent. of both A's and F's. This, however, gives too few men in the A and F classes for our purposes. If we make the range of the unit 20 per cent. smaller, we obtain the distribution shown in Figure 3, according to which of ten men four would receive C, two B, two D, one A and one F. It departs slightly from the theoretical distribution, but certainly not so much as the theoretical distribution departs from the actual distribution. It appears to be the most convenient classification when five grades are used; one in ten being given honors and one in ten being required to repeat the course corresponding fairly well with the average practise and being a convenient standard.

It is maintained by Dr. Galton, Professor Pearson and others that ability and performance are distributed in accordance with the curve of error. It does not seem to me that this is the case. If ability for scholastic work were distributed in this way at birth, it would not remain so among college students, who are a selected group. Those unfit are less likely to be found in college and those particularly competent are more likely to be there. This would tend to give us for college students a skew curve in the negative direction. In spite of this factor, I believe that the main skew is in the opposite direction,

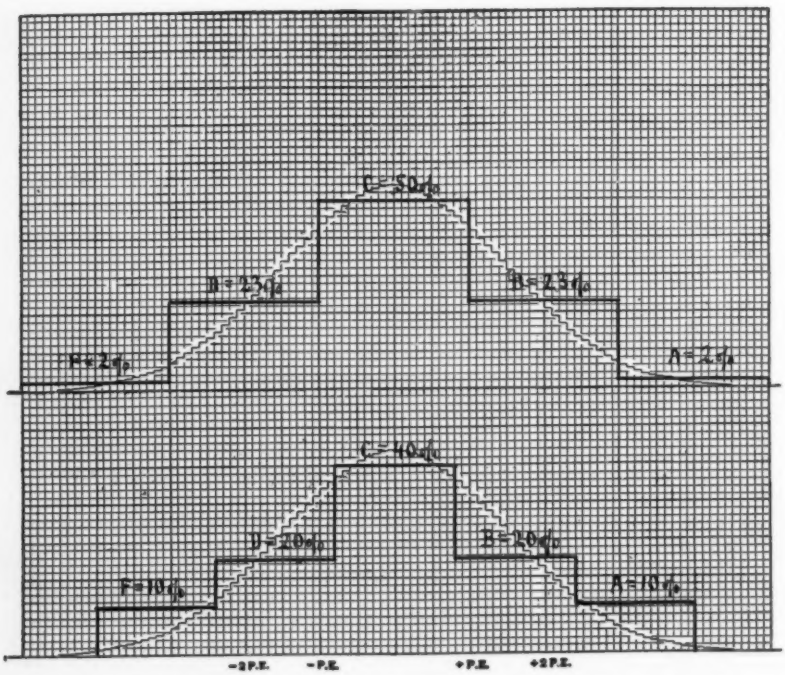


FIG. 2. THE UPPER SURFACE SHOWS THE THEORETICAL DISTRIBUTION OF GRADES, THE LOWER THAT MOST CONVENIENT IN PRACTISE.

and that ability is distributed somewhat like wages which are roughly proportional to it. If the average earnings of men in this country are $600 a year, it is clear that the positive deviations from the average are many times the negative deviations. There may be a certain minimal ability necessary for survival, and variations and sports may occur to an extent in the positive direction not possible in the negative direction. There are certain 'constant errors,' such as a college education, which divide men into different 'species.' In so far as students are graded on the lines of the probability curve, this may measure the attitude of the examiner rather than the distribution of the men in merit.

But we do not need theorizing so much as facts, which should be secured without delay. In the papers quoted above I have shown that it is possible to transform a qualitative series into one giving measures of differences. If the same thousand examination papers were read and graded independently by ten examiners, the variation in the grades of the same paper by different examiners would give us a measure of the differences between the papers, which would be inversely as the variation of the grades. I have in this way made a curve for the distribution of scientific performance in a selected group, and the same methods should be applied to merit in examinations.

In the meanwhile I am able to give the grades actually assigned in several cases. The accompanying table shows the grades given to 200 students in each of five courses in Columbia College, and the figure shows the averages and the grades in English A and Mathematics A. The average grade is a little above C, the median grade

PERCENTAGE OF STUDENTS RECEIVING

	A	B	C	D	F
Eng. A	4.5%	41.5%	44.5%	4.5%	5%
Eng. B	4	40	39	6.5	10.5
Math. A	11	24	24	22	19
Hist. A	10.5	28	28.5	20	13
Econ. A	9	36	33	17.5	4.5
Average	8	33.9	33.8	14.1	10.4

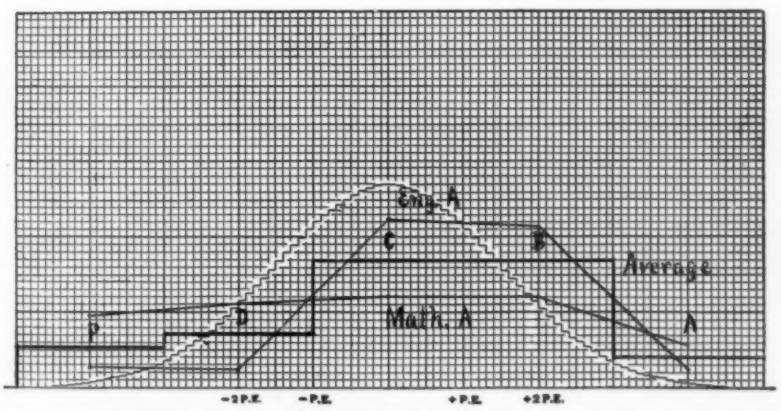


FIG. 3. THE DISTRIBUTION OF THE AVERAGE GRADES ASSIGNED IN FIVE COURSES, WITH THE DETAILS FOR INTRODUCTORY COURSES IN ENGLISH AND MATHEMATICS.

is nearly midway between C and B, and more than two thirds of all the grades are either C or B. Eight per cent. of the grades are A and ten per cent. are F, which approximates closely to the standard recommended above. The average of the grades assigned in these courses does not vary considerably, but the distribution is different. In the courses in English the distribution tends to follow the normal

curve of error, with the failures as a separate group or species. In the courses in mathematics and history the groups are more nearly equal in size, except in the case of 'excellent.' Here the range of ability is presumably greater in D and F than in B and C. The distribution in economics is intermediate. The fact that the courses in English, though given by different instructors, correspond closely shows that within a department certain standards may be followed; and this would be possible for the whole college or for the educational system of the country. It is only necessary to adopt the standards and then to teach people how to apply them.

I have also counted up the average grades assigned to 200 students in their first ten courses. In the table and curve, A represents the range between A and B + ½, B the range to C + ½, etc. Here

Average grade	A	B	C	D	F
Per cent. of students	2.5	34	46.5	16.5	0.5

the grades tend to be bunched, the differences between the men being partly obliterated by the combination of the grades in different courses.

In the next table and in the figure are given the grades of 15,275 papers assigned by the examiners of the College Entrance Ex-

Rating	100–90	89–75	74–60	59–50	49–40	39–0
Per cent. of papers	6.1	21.4	32.6	12.1	11.1	16.7

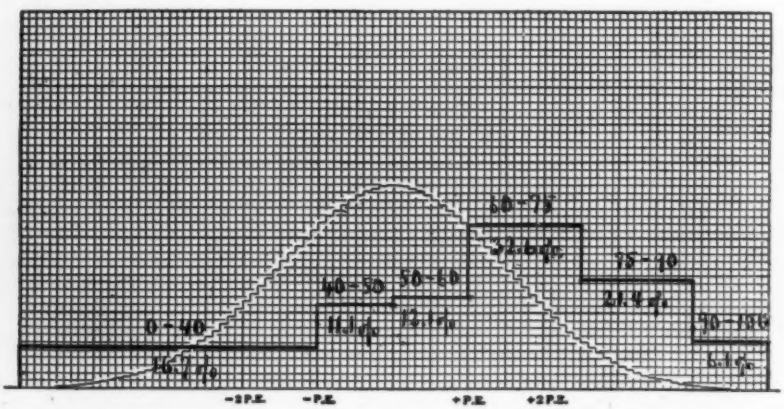


FIG. 4. DISTRIBUTION OF GRADES OF THE COLLEGE ENTRANCE EXAMINATION BOARD.

amination Board in 1904. The grades are in this case given on a centile scale. The curve is decidedly skewed in the negative direction, the most frequent grades being between 60 and 75. There is a considerable variation in the different subjects. Thus 10.6 per cent. of the candidates are given a grade above 90 in Greek and only 2.7

per cent. in history; 34.9 per cent. are given a grade below 50 in mathematics and only 19.1 per cent. in English. It is obvious that such grades should be standardized. It may be remarked incidentally that it is easy to select examiners by a competitive examination. If twenty candidates grade the same sets of papers, those whose grades are nearest the average of all the grades are likely to be the most competent examiners.

In these cases, and in all grades with which I am acquainted, there is a tendency to grade students above the average. Professor Pearson finds that in estimating the health of English boys, teachers place twice as many above 'normally healthy' as below, and he seems to regard it as gratifying that English boys should be more than normally healthy. We look on our own students as better than the average and in any case give them the benefit of the doubt. We call things 'fair' that are only average, and then the word 'fair' comes to mean average. Then we assign the grade 'fair' to students who are below the average, and a 'fair' student comes to mean a poor student. In assigning grades such words should be avoided; we should learn to think in terms of the average and probable error.

If grades are given on a centile system, the grade should mean the position of the man in his group; thus 60 should mean that in the long run it is more likely than anything else that there would be forty men better and fifty-nine not so good. The average probable error should be determined and a probable error should be attached to the grades; thus the grade 60 ± 10 means that the chances are even that there are between thirty and fifty men in the group who are better. The probable error becomes smaller as we depart from the average man; I estimate on the basis of a few experiments that it is over 10 in the middle of the scale. If this proves to be correct on the basis of more extended data, it is needless to grade more closely than on a scale of 10, though the first decimal would have some meaning when the grades are combined. If a hundred men are divided into ten groups of 10 each, the men in the middle groups will differ less from each other than those towards the ends, and if we wish to let the groups represent approximately equal ranges of merit, we can, as explained above, make five groups, A, B, C, D and F, putting 40 men in C, 20 men in both B and D and 10 in both A and F.

The determination of the validity of the grades given to college students and their standardization appear to me to be important because I regard it as desirable that students should be credited for the work they do rather than for the number of hours that they attend courses. By our present method a student who fails gets no credit at all, while a student who is nearly as bad (and perhaps worse)

gets as much credit towards his degree as the best student in the class. In our graduate faculties we credit men for work they do, and this principle is also adopted in the secondary schools that have broken the 'lock step.' Just now we hear much about the need of shortening the four year college course. Men can not do the work of four years in three by attending more courses each year, but some men accomplish as much in three years as others do in four, and many men, if they had an adequate motive, would do as much in three years as they now do in four.

We find among our graduate students that the better men can obtain the doctor's degree in about half the time required by the poorer men, while in exceptional cases the range is greater. I have found in various fundamental traits that can be measured, such as accuracy of perception, reaction-time and memory, that ordinary individuals differ about as 2:1. It seems that the best men (say the first ten) in our classes differ from the poorest (say the last ten) in about this ratio. If, therefore, men are divided into five groups representing nearly equal ranges of ability and we give the C, or middle group, a credit of three points for a three hour course, it would be just to give the A group 4 points, the B group 3½ points, the D group 2½ points, and the F group 2 points or less.

In Columbia College sixty points are required for the bachelor's degree, a point being an hour's attendance at lectures or recitations, or two hours of laboratory work. Students are expected to attend classes for about 15 hours a week and usually receive the degree in four years; there are, however, some who attend 20 hours a week and receive the degree in three years. At Harvard College 54 points are required, and I understand that about half the students now accomplish the work in three years. When 60 points are required for the degree, and if credits as proposed above were assigned, the 200 students of Columbia College whose grades have been compiled on the basis of half the work for the degree would be required to attend a total number of hours, as follows:

Grades	A	B	C	D	F
Per cent. of students	2.5	34	46.5	16.5	0.5
Hours for degree	40–45	45–55	55–65	65–75	75 +

This would be a just assignment of credits to the best of our present knowledge. It would permit about one third of the best students to secure the degree by an attendance of from 15 to 18 hours a week for three years. If, however, it is thought that this gives too great a reward for good work and too great a penalty for deficiency, the credits and deductions could be halved. This would give for these students an attendance, as follows:

Grades	A	B	C	D	F
Per cent. of students	2.5	34.5	46.5	16.5	0.5
Hours for degrees	47.5–52.5	52.5–57.5	57.5–62.5	65.5–67.5	67.5 +

Or, if the grades were standardized on the lines here proposed, the percentages would become:

Grades	A	B	C	D	F
Per cent. of students	10	20	40	20	10
Hours for degree	47.5–52.5	52.5–57.5	57.5–62.5	62.5–67.5	no degree

It would also be possible to introduce the principle of giving extra credit for good work in a less radical manner, for example, by allowing a credit of three points to students who receive the highest grade in at least five courses. The application of the principle in any form would be an important educational advance, but a method such as this would not be nearly so fair and accurate as the plan here recommended. It would affect only a few men and would be more dependent on chance. The amount of credit in the plan recommended can be so adjusted that a given percentage of students can receive any credit desired; those receiving the highest grade (the first ten per cent. in the long run) could be awarded, on the average, an extra credit of 2, 3, 5, 10 or 20 points, as may be decided, and all others would receive credits in proportion.

I see no serious objection to the plan. The aberrancy of grades in different subjects would be a drawback, but not so serious as the existence of 'snap courses' under the present system. The adoption of the plan would tend to the standardization of grades, and the apparent objection might prove to be a real advantage. If it is objected that it would lead students to work too much for grades, this would simply mean, if grades are properly assigned, that it would lead them to do better work. The present method, where the grade is simply a kind of prize or punishment putting one man before another, seems to have objections; I have some sympathy with the students who call 'C' the 'gentleman's grade.' But if grades had some real meaning, they would be no more invidious than the payment of a salary of $3,000 to one man and of $5,000 to another. If it is said that the method is unfair because grades can not be given in accordance with exact deserts, it may be replied that this is true of all salaries and the like. Although a single grade is subject to a considerable probable error, the error of the average of a number of grades decreases as the square root of the number. Thus, if the probable error of a single grade is one place (that is, if a man receives C, the chances are even that he deserves a higher or a lower grade), the average of 25 grades (about the number of college courses taken for the degree) would be subject to a probable error of only one fifth of a place. Lastly it may be

said that the bookkeeping is very simple—the credits for 400 students can be compiled by an ordinary clerk in one day.

The assignment of credits in accordance with the work done by the student rather than for the number of exercises he attends appears to be in accord with common sense and justice. If after four years' study one man has the qualifications for the B.A. and another for the M.A., each should be given his appropriate degree. It may be well for one student to attend exercises for twelve hours a week and for another to be eighteen hours in attendance, but if each accomplishes the same amount of work they should be given the same credit. The plan would prove an excellent stimulus to good work and would attract to the college that adopted it the best class of students.

I should myself not only like to give students credit for the degree in accordance with the work they do, but I should also like to see tuition fees charged in proportion. In this case conduct and character should be included as well as merit in class work. More of the endowment of the institution should be used for those whose education is the greater service to the community, while those whose presence in a college interferes with its work should not be supported at the public expense. If the tuition fee is $150, it should be apportioned as follows:

Grades	A	B	C	D	F
Per cent. of students	10	20	40	20	10
Tuition fee	$100–120	$120–140	$140–160	$160–180	$180–200

But I fear that it will be even more difficult to convince trustees than faculties that psychology is becoming an exact science.

SHORTER ARTICLES AND DISCUSSION.

DE MORGAN AND CHARACTERISTIC CURVES OF COMPOSITION.

To THE EDITOR: In his quotation from Augustus De Morgan in reference to the application of the law of averages to the detection of authorship and in his remarks thereon (see the December number of this journal) Dr. Raymond Pearl is, unconsciously of course, guilty of the exact fault which he, by implication at least, attributes to others; namely, ignorance of the work of previous writers upon the same subject. As his note seems to invite others to share with him in that ignorance, it may be desirable to explain, *once more*, that it was this very suggestion of De Morgan's that started the investigation of more than twenty years ago; that in the presentation of the first results to the American Association for the Advancement of Science, De Morgan was fully credited with the idea; in the publication of these results in SCIENCE (about 1885) indebtedness to De Morgan was distinctly acknowledged; also in subsequent publications and papers, the latest being an article in a recent number of this journal (August, 1904);—all of which can easily be seen and known by anybody who cares enough about the subject to look it up. In the first, and, as far as I know, the only thorough test of De Morgan's idea, made more than twenty years ago, it was found to be difficult, perhaps impossible, to discriminate among authors by means of simple 'Average word lengths,' as suggested by De Morgan. The scheme for the graphic display of the variations in the average frequency of occurrence of words of different lengths was then devised, proving to be a vastly more powerful means of revealing peculiarities of composition. This is the only feature of the work which has been claimed as original, and the results of an exhaustive application of it were published in 1901, confirming me in my confidence in the truth of the general principle stated by De Morgan, though not in the value of the specific application of it suggested by him.

I believe that the scheme of analysis by 'Characteristic Curves,' devised a quarter of a century ago, has been 'rediscovered' one or twice since; but it should never be overlooked that the germ of the thing was in a brief remark that I found in that now, but never-ought-to-be-out-of-print book, the 'Budget of Paradoxes.' The 'Memoir' from which the letter is quoted by Dr. Pearl did not appear until many years later. As I now remember it, the original suggestion was much less elaborated than in the letter, which I think I have never seen before.

I do not understand why Dr. Pearl calls this the 'Sherman Principle.'

T. C. M.

ROME, ITALY,
December 17, 1904.

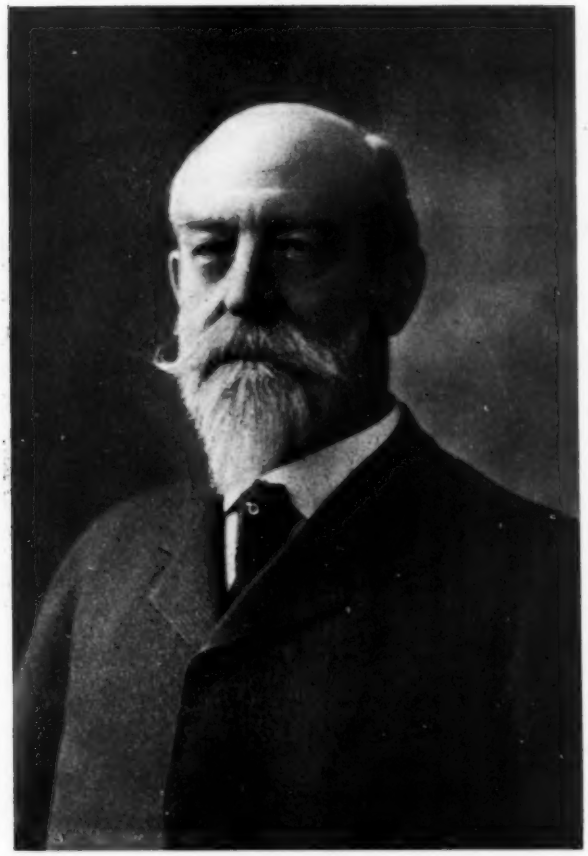
THE PROGRESS OF SCIENCE.

CONVOCATION WEEK IN PHILADELPHIA.

THE American Association for the Advancement of Science has been fortunate in its associations with Philadelphia. In our last issue we called attention to the fact that its first meeting was held there in 1848, and it was there again in 1884 that the registered attendance reached its maximum of 1,261, including 303 representatives of the British Association. If we include the members of societies which met in affiliation with the association at the third Philadelphia meeting, held during convocation week in December, the number of scientific men in attendance must be estimated as about 1,200, although only 581 members registered under the several sections of the association. Perhaps there has never been a larger gathering of American workers in science. More significant than mere numbers is the representative character of the men in attendance, the spirit of the convocation as a whole and its influence upon the general public. In all these respects the recent meetings at Philadelphia were eminently successful. There were, in addition to the nine sections of the association which were in session, thirty affiliated societies and scientific clubs, including a majority of the national societies in the exact and natural sciences. More than 500 papers were read, covering even a wider range of topics than the names of the societies would indicate.

The societies were comfortably accommodated in the beautiful buildings of the University of Pennsylvania, many of which were independent objects of interest to visiting members. The material conveniences were but typical of the hospitality which the several societies enjoyed at the hands of the provost, vice-provost and faculty of the university and the local committee. A feature in the entertainment of guests was the lunch provided daily for all in attendance. There were the usual receptions, smokers, etc., and excursions to many places of interest to all scientific men or to special groups, including such important local institutions as the Baldwin Locomotive Works, Cramp's Ship Yard, the United States Mint and the Navy Yard.

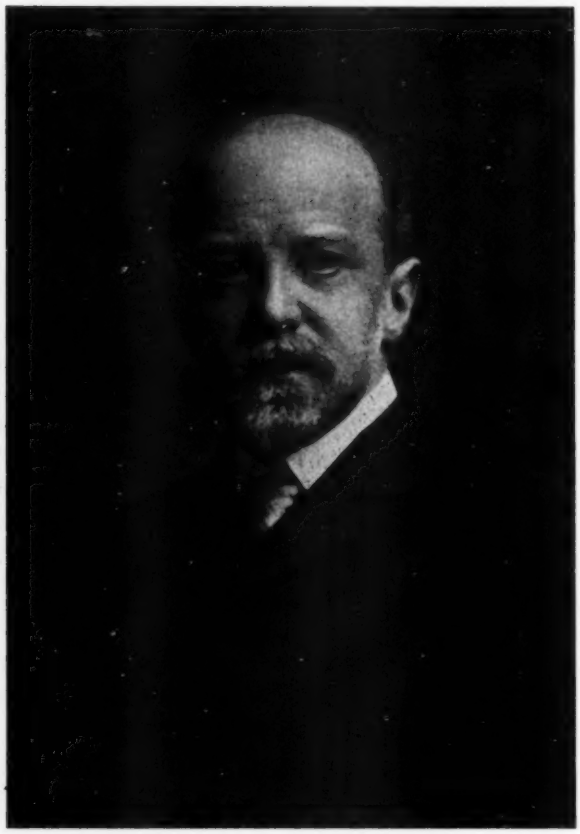
The number and variety of papers of popular interest was notable, including discussions of some of the most promising applications of science in invention and industry, in agriculture, in medicine and in social economics. All the societies were fortunate in that the American Association had secured the services of Mr. Theodore Waters, as press secretary, in order to insure more adequate reporting of the meetings than has been possible hitherto. This is an important consideration, as it is assuredly one of the purposes of the association to impress upon the general public the dignity and importance of science. Modern civilization is increasingly dependent on the progress of science, and men of science must profit by the sympathy and support of the public benefiting by their labors. Owing partly to geographical conditions, partly to inadequate organization, though doubtless not wholly to these causes, there is here less public interest and general participation in the annual meetings of the American Association than is the case in England with regard to the British Association. It is evident, however, that our public inter-



H. P. BOWDITCH,

Professor of Physiology, Harvard Medical School, Vice-President for Physiology and Experimental Medicine.

est not only comes short of what it should be, in view of the real importance of science in our life and the possible value of the association to the nation, but it at present comes far short of what it is destined to become, we venture to hope, in the not distant future. All sympathizers with science, that is, all intelligent sympathizers with civilization, should be enrolled as members of the association. The present membership of about 4,000 should be speedily doubled, if not tripled, if the association is to do the work for which it is fitted. Its growth has been steady, but not commensurate with the growing needs of American science. Since the last meeting 377 new members have been added. Especially desirable is it that all scientific workers be members of the association as well as of their respective technical societies. It may not be generally known that any scientific society so desiring may, by vote of the council, become affiliated with the central association, without in the least sacrificing its autonomy, thereby gaining not only the advantage of profiting by the arrangements for the meetings which the association undertakes, but securing representation in the council in proportion to the number of its members enrolled as fellows of the association.

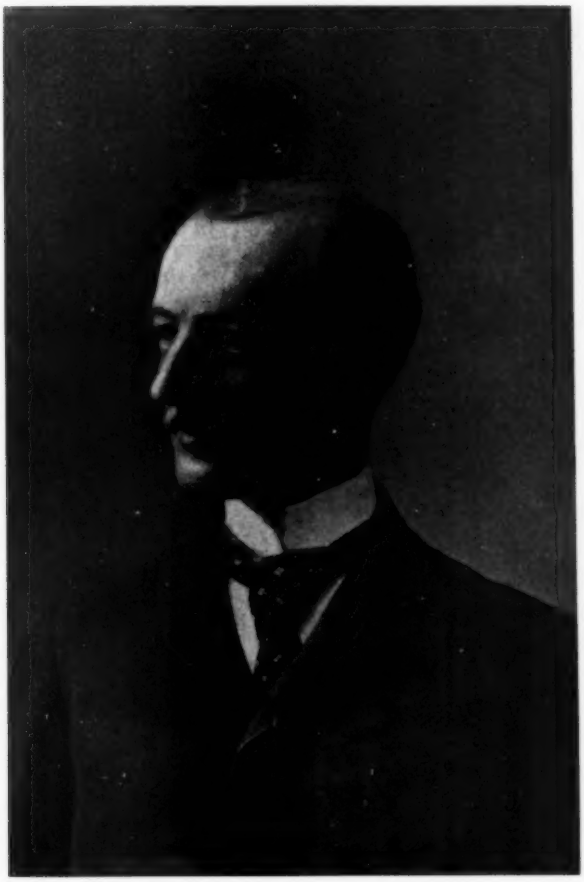


LEONARD P. KINNICUTT,

Professor of Chemistry, Worcester Polytechnic Institute, Vice-President for Chemistry.

THE AMERICAN ASSOCIATION.

The fifty-fourth meeting of the American Association was presided over by Professor William G. Farlow, the eminent botanist of Harvard University, who responded to the address of welcome tendered by Provost Harrison of the University of Pennsylvania. We are pleased that we are able to give here portraits of the vice-presidents who presided over the sections and of the permanent secretary and the secretary of the council. No report of the sectional meetings can be attempted, and only a brief account of the more important business proceedings. In some respects this is a critical period in the development of the association. With the increasing specialization of methods and of the sciences, a number of special societies of experts have arisen, which tend to replace, in a way, the older and more general association. Just what relation the latter should bear to the several societies affiliated with it is an important question of policy awaiting early solution. It is thought by some that the association should aim to serve as a central legislative body and to coordinate and represent to the public at large the common interests of the several special bodies, while yielding to these the sessions for the reading of technical papers. The Philadelphia convocation was notable



DAVID S. JACOBUS,

Professor of Experimental Mechanics and Engineering Physics, Stevens Institute of Technology, Vice-President for Mechanical Science and Engineering.

for the harmony which prevailed between the special societies and the general association. In several cases the sections had charge of a general session in one half the day, and the affiliated societies of a meeting during the other half. Those sections of the association with which affiliated societies met were naturally the most largely attended. In regard to other questions, such as time and place of meeting, a certain amount of conflict of interests is bound to occur. It may be desirable and feasible for the association to hold two meetings annually, one during convocation week, primarily in the interests of scientific organization in America, another in the summer, somewhat more along the old lines familiar to our readers. These and other vexing questions of policy come up at Philadelphia, and were reported on by the committee on the policy of the association. This committee was empowered to exercise a general executive control of the preliminary arrangements for meetings and of the publications, and President R. S. Woodward, of the Carnegie Institution, was appointed as its permanent chairman, continuity and responsibility being thus insured.

The national character of the asso-

WM. F. MAGIE,

Professor of Physics, Princeton University, Vice-President for Physics.

ciation was emphasized by the selection of New Orleans as the place of meeting during next convocation week, beginning on December 29, 1905. Boston was recommended as the place of meeting for 1906. It was also recommended that a summer meeting be held in Ithaca in 1906. Professor Calvin Milton Woodward, of Washington University, St. Louis, was elected president of the association for the New Orleans meeting. Professor Woodward was born at Fitchburg, Mass., August 25, 1837. He is a graduate of Harvard (1860) and a doctor of philosophy from Washington University (1883). He has occupied the chair of mathematics and applied mechanics at the Washington University since 1870, and for many years he served as dean of the school of engineering. In 1879 he originated the St. Louis Manual Training School, of which he has been director ever since. Dr. Woodward has been an active member of the St. Louis school board and the president of the board of regents of the Missouri State University. He has written important books on manual training in education. A member of the association since 1883, he has been interested in the work of three of its sections, those of mathematics and astronomy, of mechanical science and engineering, and

B. L. ROBINSON,

Asa Gray Professor of Systematic Botany, Harvard University, Vice-President for Botany.

of social and economic science, having been secretary for the last two, and at different times elected to the vice-presidency of the same. At Philadelphia he gave the vice-presidential address before Section D on 'Lines of Progress in Engineering.' His portrait will be found in the issue of the MONTHLY for February, 1904. Space does not permit more than a list of the well-known names of the newly elected vice-presidents, which are as follows:

Section of Mathematics and Astronomy—Professor W. S. Eichelberger, Washington, D. C.

Section of Physics—Professor Henry Crew, Evanston, Ill.

Section of Chemistry—Professor Charles F. Mabery, Cleveland, Ohio.

Section of Mechanical Science and Engineering.—Professor F. W. McNair, Houghton, Mich.

Section of Geology.—Professor William North Rice, Middletown, Conn.

Section of Zoology.—Professor H. B. Ward, Lincoln, Nebr.

Section of Botany.—Dr. Erwin F. Smith, Washington, D. C.

Section of Anthropology.—Dr. George Grant McCurdy, New Haven, Conn.

Section of Social and Economic Science.—Professor Irving Fischer, New Haven, Conn.

Section of Physiology and Experimental Medicine.—Professor William T. Sedgwick, Boston, Mass.

C. HART MERRIAM,

Chief of the U. S. Biological Survey, Vice-President for Zoölogy.

Dr. L. O Howard, of Washington, D. C., was reelected for a term of five years, as permanent secretary; Professor C. A. Waldo, of Lafayette, Ind., as general secretary and Professor John F. Hayford, of Washington, D. C., as secretary of the council.

THE PRESIDENTIAL ADDRESS.

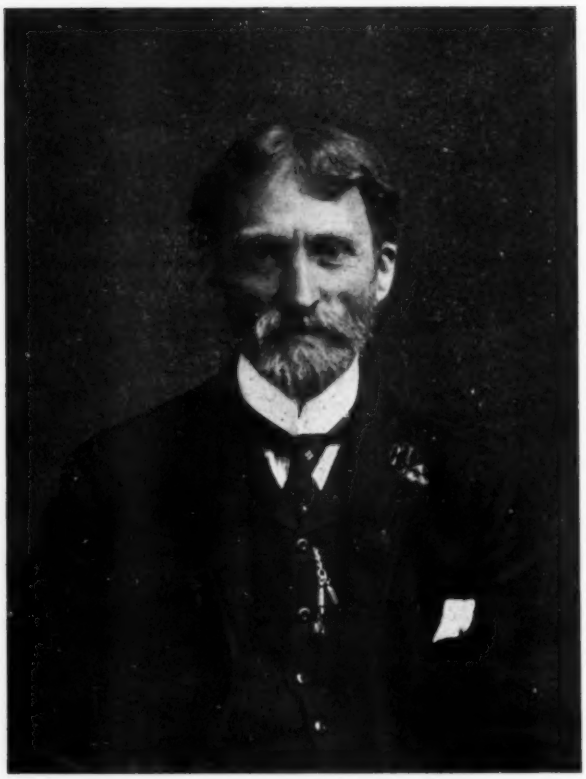
The address of the retiring president, Dr. Carroll D. Wright, formerly U. S. Commissioner of Labor, now president of Clark College, was an event of unusual significance. Dr. Wright, as is well known, is an economist, and he chose for his subject 'Science and Economics.' After referring to the revolutionizing influence of science in the realm of theology and religion, President Wright pointed out a similar influence, not as yet generally appreciated, in the sphere of political economy. The subject was, properly, presented in broad, general outlines, touching as it does a wide range of topics, such as the Malthusian theory of population, the law of diminishing returns, the iron law of wages, and the like, and even the tariff, together with such matters as the relation of chemistry and engineering science to problems of national and international politics, and the

EUGENE A. SMITH,

State Geologist of Alabama and Professor of Geology in the University of Alabama, Vice-President for Geology and Geography.

physiological and psychological conditions of efficiency as factors in the labor problem. It was shown how the progress of science has resulted in the abandonment or modification of the hard and fast theories of the classic school of political economists and the substitution of more flexible formulas, suited to the number and variety of facts brought to light by newer methods of investigation not only in economic and social science, but in the so-called exact and natural sciences as well. Modifications of old 'laws' also become necessary as the result of the progress of applied science, the extensive use of machinery, *e. g.*, tending to modify the operation of the 'law of diminishing returns.' Agricultural science, or more accurately, the agricultural sciences, stand in the closest relation to the economic situation in any civilized country, and consequently not only to the science of economics, but to governmental policy.

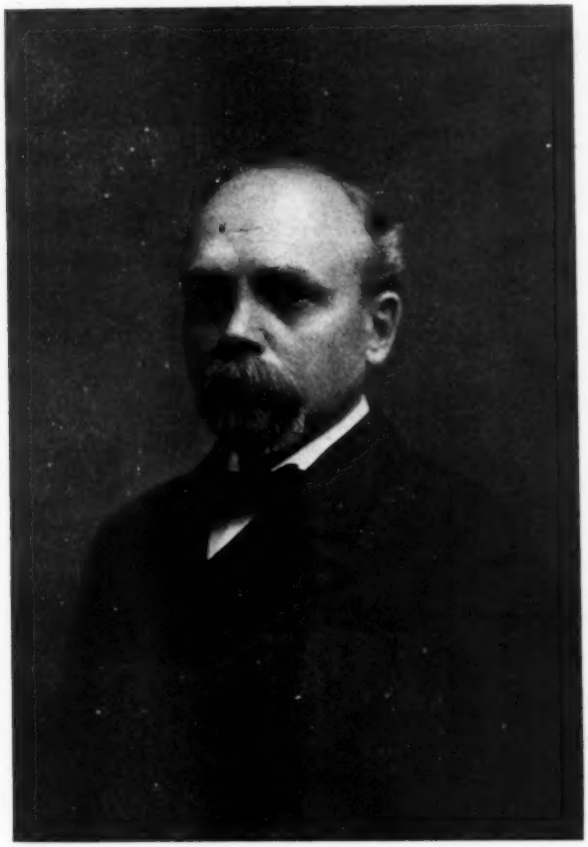
President Wright's address was typical of the interests and objects for which the association exists. THE POPULAR SCIENCE MONTHLY has consistently maintained that science is not a thing apart, the curious pastime of an exclusive clique, but that it is nothing more nor less than the most complete and exact statement of fact that can be reached by the most careful and thorough methods of inquiry, and that



WALTER HOUGH,

Assistant Curator, Division of Ethnology, U. S. National Museum, Vice-President for Anthropology.

its results belong not to the few, but to the many, apply not in the domain of industry and our material civilization alone, but over the whole range of life's activities, are not for individual advantage chiefly, but for the public good. When society in general and governments in particular appreciate the true source and guardian of their highest welfare, then many things now desired, and many more no less needful, will be possible of attainment. It does not follow that because nations have for the most part stumbled along through a process of trial and error without planning their trials or measuring the significance of their errors, that they will always entail such waste in a needlessly blind pursuit of progress. The world proceeds by gradual evolution, yet, as we have often insisted and again repeat, this time in the words of Mr. John Morley, 'evolution is not a force, but a process; not a cause, but a law.' Ideas may be forces, purposes may be causes, and intelligent cooperation and organized effort may minister to the economy of social progress as they have to the promotion of success in the whole world of private business. The like principle holds for the internal organization of scientific men and scientific bodies themselves. There must be a solidarity of sympathy and aims among scientific workers if total efficiency, and not merely partial efficiency here and there, is their aim. When the forces of



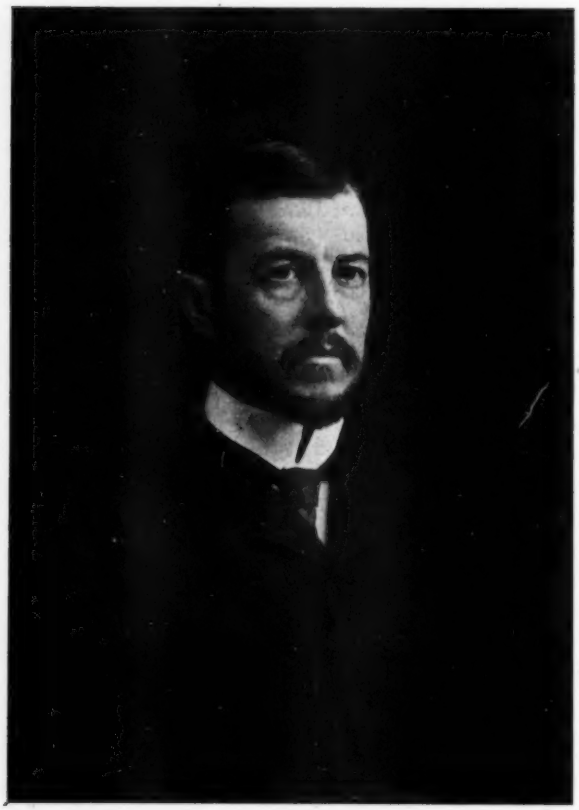
MARTIN A. KNAPP,

Chairman of the Interstate Commerce Commission, Vice-President for Social and Economic Science.

science shall have become satisfactorily organized within, then it will be possible for science as a whole to take its proper place in public affairs.

THE AFFILIATED SOCIETIES.

Among the important societies which met with the association were the American Chemical Society, under President Arthur A. Noyes; the American Physical Society, with President Arthur A. Webster in the chair; the Astronomical and Astrophysical Society of America, presided over by Professor Simon Newcomb; the Geological Society of America under President John C. Branner; the newly organized American Geographers' Association, Professor W. M. Davis presiding; and the American Society of Naturalists with its several affiliated societies, including those devoted to botany, zoology, anatomy, physiology, bacteriology, anthropology, psychology, philosophy, etc. Notable too was the interest shown in the vigorous societies devoted to the application of science, in agriculture, horticulture, entomology and other lines. The meeting of the American Chemical Society, the first to affiliate with the association, was particularly successful, 240 chemists having registered. It was necessary to subdivide into smaller sub-



CHARLES S. HOWE,

President of the Case School of Applied Science, Cleveland, Ohio; General Secretary o the American Association.

sections for the reading and discussion of the sixty odd papers that were presented. Under the auspices of the Naturalists, Professor Henry Fairfield Osborn gave an interesting illustrated lecture in the lecture hall of the famous old Academy of Natural Sciences, his subject being 'Recent Discoveries of Extinct Animals in the Rocky Mountain Region, and their Bearings on Present Problems of Evolution.' 'The Mutation Theory of Organic Evolution' was discussed in a series of specially prepared papers by representatives of the most advanced research in their respective fields; plant breeding being represented by Dr. D. T. MacDougal, animal breeding by Professor W. E. Castle, cytology by Professor E. G. Conklin, paleontology by Professor W. B. Scott, anatomy by Professor Thomas Dwight, taxonomy by Professor Liberty H. Bailey and ethology by Dr. W. M. Wheeler. This was one of the best discussions that has been held before the Naturalists, as generally interesting as it was timely. It illustrates the value and need of comparison and coordination of the results of investigation in different fields for the solution of problems common to all. If methods are becoming more and more specialized with the growth of exactitude in the sciences,

L. O. HOWARD,

Chief of the Division of Entomology, U. S. Department of Agriculture, Permanent Secretary of the American Association.

there is at the same time a growing class of problems which may be termed synthetic, to which special students must devote themselves without reference to the traditional boundaries of the separate sciences. These include the problems of evolution and many of the most pressing problems of solar research, geophysics, and the like, and still more of such applied sciences as medicine, agriculture and engineering. This suggests one of the strongest reasons, appealing to the interests of pure research itself, for a common meeting-ground of investigators in artificially separated though not unrelated fields, which shall serve as a clearing house for different classes of students at work on the same problems.

Professor William James, the eminent psychologist, who made a notable presidential address before the psychological association this year, was elected president of the American Society of Naturalists for the next meeting. There will inevitably be some separation of the societies next year, but it is hoped that all will be reunited for an especially good general meeting at Boston the year following.

SCIENTIFIC ITEMS.

THE Nobel prize for physics has been awarded to Lord Rayleigh, professor of natural philosophy at the Royal Institution. The chemistry prize is conferred upon Sir William Ramsay, professor of chemistry at University College. M. Pavloff, professor at the Military Academy of Medicine at St. Petersburg, receives the prize for physiology and medicine. The literature prize is divided between M. Mistral, the Provençal poet, and Don Jose Echegaray, the Spanish dramatist. The peace prize has been awarded to the Institute of International Law.

MR. LUTHER BURBANK, whose important work on plant-breeding was described by President Jordan in the last issue of the MONTHLY, has been appointed a special lecturer at Stanford University. He has received a liberal grant from the Carnegie Institution, which will permit him to devote himself to scientific work—Professor Svante Arrhenius has been made head of a laboratory for physical chemistry, to be established at Stockholm by the Nobel Institute.—Dr. Horace Jayne has resigned the directorship of the Wistar Institute of the University of Pennsylvania.

PROFESSOR S. W. BURNHAM, astronomer at the Yerkes Observatory, has been awarded the Lalande gold medal of the French Academy of Sciences for his researches in astronomy.—The Lavoisier medal has been awarded to Sir James Dewar.—Professor G. Sergi has been made president for the International Congress of Psychology to be held at Rome from April 26 to 30 of the present year.—Lord Kelvin has accepted the nomination of the council for the presidency of the London Faraday Society, in succession to Sir Joseph Swan.